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AND THE RECORDING OF

AMERICAN AND OTHER PATENTED INVENTIONS.

SEPTEMBER, 1837.

Practical and Theoretical Mechanics and Chemistry.

Notices of various things in existence, or in progress; Extracted from a Letter addressed to the Editor, by Jacob Perkins, Esq., London.

In publishing some communications recently received from our old friend Perkins, we observed that the gentleman, in whose hands they had been placed, had been long detained, and that the information furnished was nearly a twelvemonth old. The letter from which the subjoined extracts are taken, made a part of the papers received, and the Editor hopes that some of the interest felt by him on the matters to which it relates, may still be excited in the minds of his readers.

"You no doubt, in common with many others, have wondered at my long delay in producing some things so confidently promised in my communications to you, and in certain public announcements, made some years since. The particular causes of this delay are too much of a private nature to justify, or warrant, their detail, and I will simply state, therefore, that the extravagant demands made upon me in consequence of the insolvency of persons with whom I have been connected, involved me in law, and that the seal of Chancery was placed even upon my unfinished experiments; this however, is now broken, and since I have been able to resume them, I have compelled the most inveterate of my opponents to acknowledge that I have struck upon some veins in the mines of scientific knowledge well worth

exploring. I send you by the bearer, Col. Petival, a model of my new steam-boiler, or rather generator, which I am about to patent both in this country, and in the United States; so far it has realized my most sanguine expectations both as regards economy and safety. The pamphlet which accompanies it [not received] will explain my general views, but the most interesting part of this improvement must remain unpublished until the respective patents are secured.

The paper which I have sent you on the application of the Pneumatic Engine to railway purposes, looks, I must acknowledge, a little *Redhefferish* in some of its parts; it, however, relates actual facts, and will be applied in a manner which will become the subject of patents in the United States.

You still hear from this side of the water accounts of the running, or intended running, of steam carriages upon common roads. I am aware that you have repeatedly expressed an opinion that they could not succeed. This, from the first attempt made, up to the present time, has also been my settled conviction: that is, that they never can so run as to compete with rail-roads. But notwithstanding the numerous failures and abandonments, much money will yet be spent before the project will be given up. There are now two splendid steam tugs to start in a few days, with their attached omnibuses; they will undoubtedly run for a short time, after which they will be withdrawn and soon pass into the shades. This grand concern was patronized by the late celebrated engineer Tellford, who as a constructor of canals, bridges, turnpike roads, &c., &c., has established a fame which will be imperishable. My attachment to him was very great; he was a man to be loved; was extremely liberal in all his views and actions; and to this country his loss is a very heavy one. Had he lived a few years longer, he, I am well assured, would have experienced severe mortification in witnessing the entire failure of his favourite system of steam-coach traveling, which was eventually to supersede the rail-road system. There are, you know, spots upon the sun, and probably upon every other luminary.

I intend shortly to send you a drawing of my ice making machine. It is very simple in its construction, and economical in its operation, so much so that I can make ice from filtered water at a cost of about one shilling per cwt., which does not exceed one-eighth of its market value. This estimate is made under the supposed application of steam-power, manufacturing in the large way. When made by hand its cost will be about a penny per pound.

I have lately obtained a patent for a centrifugal locomotive blower, which gives a much stronger blast than the ordinary revolving blower, when of only one half the size. One of its important features is its giving the blast without noise. The air is drawn in at one side, and is driven out at the other, not at the periphery, as heretofore; every fan is at work throwing the air over the disk, which is equal in diameter to the fans, on the delivery side.—Many blowers which have been put up for steam-boats, cupola furnaces, &c., have been taken down on account of their trumpet-like noise, although they were otherwise very efficient. I intend to try them for a new purpose in steam-boats, namely, to dispense with the huge chimney, and to throw the exhausted air and smoke into the paddle case. Speaking of the paddle case, reminds me of my oblique paddle-wheel, so well understood, and explained by you in your Journal; it has not yet been tried on a large scale, but in all my experiments in the small way it has performed well. I had a boat fitted on one side with the ordinary paddle-wheel, and on the other with my oblique paddles, each exposing an equal surface, and being equal in diameter, the rudder being fixed in a line with the keel; when the

wheels were made to revolve, the boat moved in a circle of small radius, showing the great advantage in favour of my wheel. I still hope to live long enough to try it on a large scale on my return to my own country, which I intend shall be at an early day. To you it may appear strange, but it is true notwithstanding, that long as this wheel has been before the public there are very few indeed, even among professed engineers, who understand its true principle.*

The Adelaide Gallery, of which you have frequently heard, continues to attract attention. When I first proposed the establishment of such an exhibition as that which is made there, its success was a subject of much doubt. It is now, however, firmly established. Its average number of visitants is 300 per day, each paying one shilling for admittance; on some occasions there have been 1000; and the visitors are of the most respectable class. My steam gun showers its balls every hour from 12 to 4 o'clock. The gallery opposite to this gun is 150 feet long, and it is frequently filled three or four deep, with intelligent spectators, and although it has been thus in use for three years it still continues to be the lion of the day. Foreigners who visit the gallery not unfrequently avow that the object of their journey from the continent was to see this gun. On a recent occasion just before the last volley was discharged, the atmosphere became suddenly very dark, and many were waiting to see the last shower of balls; the steam was somewhat higher than common, the gun had been recently lengthened, and from the concurrence of these circumstances the velocity of the balls was much increased, and a very remarkable effect was produced. When the balls came into contact with the cast-iron target, a very brilliant group of stars was seen on the plate, each about the size of a marygold, and somewhat resembling it in appearance; light was seen also at the mouth of the gun; and the leaden balls were completely pulverized. What could have produced these effects? was it not the result of the development of electricity by the friction, or from some uninvestigated cause?

The action of a soft iron disk upon hard steel, such as a file, is exhibited four times a day. This has been regularly kept up for three years, yet it has undergone very little wear. I am of opinion, in fact, that if the file had never been held upon it until it had attained its full velocity, there would not have been any loss of metal. I do not know to what extent the combustion of steel by soft iron may have been carried in the United States, but our experiments are so brilliant as to excite the highest admiration, and to induce numbers to repeat their visits to the Gallery. Our disk is a foot in diameter and an eighth of an inch thick. It requires about a three horse power to drive it, and revolves about 6000 times in a minute. It is very accurately fitted up with friction wheels. The blaze of light which rises about 12 inches, perpendicularly, from the point of contact, is so vivid that but few persons can look steadily at it even at noon day. The stream of light is about an inch and a half thick at the distance of a few inches from the point of contact, and at the distance of 7 or 8 feet it spreads out to about 10 inches. The sparks not unfrequently touch the ceiling, which is about 20 feet high; a ring of fire is seen all around the disk, appearing like a band of light about five-eighths of an inch wide. Of what does this light consist? It is manifestly different from that of the sparks, which all fly off

* A gentleman recently from England, an Engineer by profession, and a frequent visitant at the Adelaide Gallery in London, where the model is daily exhibited in operation, denied the possibility of the main thing which characterizes it, when mentioned to him by the editor, yet professed to know every thing about it. See Vol. V, p. 334, of this Journal.

in a tangent. In operating with the disk, it never becomes warm; the file, however, has to be held at least two inches from the sharp end, as it becomes highly heated. The whole appearance, in fact, is very interesting, and when fully investigated, I am well convinced that some of the phenomena will be found to depend upon electricity.

My son Anger is doing well with his patented apparatus for heating by hot water. The system of heating by hot water and steam is being abandoned, and the old apparatus removed from various public buildings to give place to the new. This is the case in the British Museum, where Anger has given great satisfaction. He has fitted up two ships for the North Pole, and orders crowd upon him faster than he can execute them. A thousand reports of explosions, fires, &c., have been invented and propagated, as caused by his system, the whole of which are utterly false, and the intended evil is recoiling upon the interested individuals who have aimed the blow at him. The apparatus has already worked well in various buildings during successive years without losing a drop of water. The filling pipes are regularly examined once or twice a week, when, owing to some invisible leak, it may, for a time, require a few pounds of water, but oxidation soon fills up any such small opening, sealing it perfectly. It would seem incredible, but it is a fact, that in this apparatus a stream of hot water which is not thicker than your little finger, carries the heat 700 feet, and then returns to the boiler at the temperature of 100 degrees. The boiler is a coil of tube of the same diameter with the conducting pipe. From the extent of that magnificent building, the British Museum, the expenditure this year, on the heating apparatus, will be upwards of $6000.

Prior to the ascertaining the proper strength for the pipes, in the early trials, there were a number of explosions, but not such as produced any injury beyond that to the pipe.

I have several other things on the tapis, which it is my intention to communicate to you at an early day. For some of them I am preparing to obtain patents, which will soon be matured. Yours, &c.

J. Perkins.

Physical Science.

Igniting Gunpowder by Electricity.

An easy method of Igniting Gunpowder by common Electricity, by Chas. P. Page, M. D. of Salem, Massachusetts.

Having often witnessed the futile attempts of experimenters to ignite gunpowder by small Leyden jars, I presume the following communication may be of value. I have been in possession of the fact for six years, but have never been able to fix upon a rationale. In common experiments for igniting gunpowder by electricity it is found necessary to introduce iron filings, or detonating powder, or to use very large jars, or batteries. But by a simple arrangement, powder may be exploded by a half pint or smaller jar; and what appears most singular in the experiment, there is no audible discharge of the jar; and when examined after the experiment, it is found still highly charged. An effectual method which has long been in use is that of passing the charge through water, but in this case also large jars are requisite. If a piece of wetted cotton thread be introduced in any part of the circuit, the powder may be ignited by a few inches of coated surface. The experiment is best regulated by Lane's discharging electro-

meter, and the length of the thread should be in proportion to the distance between the ball of the electrometer and that of the jar; that is, the thread should be so long that the jar cannot be discharged over its surface. From the curious result of this apparently retarded discharge, it was at first thought that it might have assumed somewhat the character of a galvanic current. But though frequently examined by the galvanometer and fine platinum wires, I have not detected any analogy.

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We give place with pleasure to the above communication, although the experiment of restricting the electric current to a moistened thread for the purpose of more easily firing gunpowder is not new to us. It has been practised also by several experimenters of our acquaintance; among whom are Dr. Hare and Professors Johnson and Green, of Philadelphia, and doubtless by many others conversant with the progress of experimental science. We delayed inserting the paper until we could refer the author to the places where he may obtain more precise information on the interesting facts to which he refers.

In the 67th vol. of the Philosophical Magazine is a paper by W. Sturgeon, describing the difficulties he had met with, and claiming the discovery of the moistened thread. He mentions several variations of the experiment. Two years before this, Charles Woodward in a short paper in the 7th vol. (new series) of Thompson's Annals, showed an improvement on Cavallo's method of firing gunpowder by using small tubes containing water, as part of the circuit, and ascertained the instructive fact that it was immaterial whether the water is on the positive or negative side of the powder to be inflamed. In the 68th volume of the Philosophical Magazine, Thomas Howldy proves that powder may be inflamed by using a wooden point in the discharge, and that the gunpowder will explode even when spread on a cake of ice. Sturgeon, in the first volume of the Philosophical Magazine and Annals, comments upon the papers of Woodward and Howldy, and in the same volume is a rejoinder by Howldy. The statements of these gentlemen lead to the conclusion that when an electric discharge fails to ignite gunpowder, it is, in general, because the mechanical force of the current acting on the air disperses the grains before they are ignited, and that by retarding the velocity of the current, in causing it to pass through a filament of water or other imperfect conductor, the heating effect of the current becomes much greater in the immediate line of its track, and thus by its confined and sustained action becomes sufficient, though in very small *quantity*, to inflame the sulphur and ignite the powder. It may be that this is the sole explanation which the facts require. We have yet however much to learn in relation to the nature of electrical agencies. Dr. Faraday has shown, in the third volume, page 353, of the London and Edinburgh Philosophical Magazine, that in the cases alluded to, (the passage of common electricity through a moistened thread) there is a galvanic action fully sufficient to affect the needle of a galvanometer. The young electrician will find the several papers we have here cited worthy of his attention. G.

Progress of Practical and Theoretical Mechanics and Chemistry.

Suggestion for the use of the Blowpipe by working Miners. By John Paideaux, Esq.

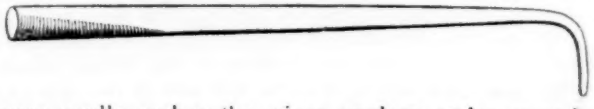
It is a leading principle of the present day, that artisans of every kind should be instructed, by means of Mechanics' Institutes, or other mediums

of communication, in the scientific, as well as practical, elements of their respective arts: a principle, the benefits of which not being confined to its particular objects, must necessarily, in thus multiplying operative intelligence, advance the arts and manufactures of the country.

In conformity with this principle, I am induced to offer some suggestions for the easy and effectual application of the blowpipe to metallurgy, by working miners; a class of men who have hitherto received but little assistance from the scientific world. If the practice should gain ground amongst them, the satisfaction of the individual workman, in understanding the quality of every ore he encounters, and in having it in his own power to examine any mineral he may suspect to be metalliferous, is but one part of the advantage that may be anticipated. When it is remembered that the grey sulphuret of copper, the richest ore of that metal, was, until of late years, thrown on the mining heaps, for mundic; and that even all the copper ores were rejected for centuries, by the tinners, as worthless, how many other substances may we not hope to save from the waste heaps, or the deads below, when the thousands of persons engaged in working the mines shall have their curiosity and cupidity thus excited?

The simple instruments and materials hereafter described, will be easily procured and generally sufficient. The miner who desires further instruction may have recourse to a little book, containing, in small compass, and at small cost, much valuable information, "Griffin on the Blowpipe;" or the more scientific work of Berzelius, translated by Children, which, however, requires more chemical knowledge.

For ordinary metallurgic assays, the common blowpipe does very well: a mere tapering tube ten inches long, half an inch diameter at one end, and the opening at the other scarcely equal to admit a pin of the smallest kind, the smaller end curved off, for $1\frac{1}{2}$ inch, to a right angle. A bulb, at the bend, to contain the vapour condensed from the breath, is useful in long operations, but may generally be dispensed with. In selecting the blowpipe, the small aperture should be chosen perfectly round and smooth, otherwise, it will not command a good flame.



A common candle, such as the miner employs under ground, answers very well for the flame.

To support the subject of assay, or "the assay," as it has been happily denominated by Children, two different materials are requisite, according as we wish to calcine or to reduce it. For the latter purpose, nothing is so good as charcoal; but that from oak is less eligible, both from its inferior combustibility, and from its containing iron, than that from alder, willow, or other light woods.

For calcination, a very convenient support, where platinum wire is difficult to procure, is white baked pipe-clay, or china clay, selecting such as will not fuse, nor become coloured, by roasting with borax.

These supports are conveniently formed by the process of Mr. Tennant. The clay is to be beaten to a smooth stiff body; then a thin cake of it, being placed between a fold of writing paper, is to be beaten out with a mallet, to the thickness of a wafer; and cut, paper and all, into squares of $\frac{3}{8}$ inch diameter, or triangles about the same size. These are to be put into the

bowl of a tobacco-pipe, and heated gently till dry; then baked till the paper is burnt away, and the clay left perfectly white. They should be baked in a clear fire, to keep out coal-dust and smoke as much as possible, as either of these, adhering to the clay plates, would colour the borax in roasting. A small fragment of the bowl of a new tobacco-pipe will serve instead, in the absence of a more convenient material.

A simple pair of forceps, to move and take up the hot assay, may be made of a slip of stiff tinplate, 8 inches long, $\frac{1}{2}$ inch wide in the middle, and one-sixteenth of an inch at the ends.

The tin being rubbed off the points on a rough whet stone, the slip is to be bent until the ends approach each other within $\frac{1}{4}$ an inch, and the two sides are parallel; thus there will be spring enough in the forceps to open and let go the assay, when not compressed upon by the finger and thumb.

A magnetic needle, very desirable to ascertain the presence of iron, is easily made, of the requisite delicacy, where a magnet is accessible. A bit of thin steel-wire, or a long fine stocking needle, having $\frac{1}{4}$ inch cut off at the point, is to be heated in the middle, that it may be slightly bent there; thus:

While hot, a bit of sealing-wax is to be attached to the centre; and the point which was cut off, being heated at the thick end, is to be fixed in the sealing-wax, so that the sharp end may serve as a pivot, descending about $\frac{1}{8}$ inch below the centre: taking care that the ends of the needle fall enough below the pivot to keep it from overturning. It must now be magnetised, by sliding one end of a magnet, half-a-dozen or more times, from the centre to one end of the needle; and the other end, a similar number of times, from the centre of the needle to its other end. A small brass thimble (not capped with iron) will do for the support; the point of the pivot being placed in one of the indentations, near the centre of the top; when, if well balanced, it will turn until it settles North and South. If one side preponderate, it must be nipped until the balance be restored.

A black gun-flint is also occasionally used to rub the metallic globules (first attached, whilst warm, to a bit of sealing-wax,) and ascertaining the colour of the streak they give. Thus minute particles of gold, copper, silver, &c., are readily discriminated.

A little refined borax and carbonate of soda, both in powder, will complete the requisites.

Having collected these materials, the next object for the operator is, to acquire the faculty of keeping up an unintermitted blast through the pipe, whilst breathing freely through the nose.

This is easier to learn from example, than from written instructions; yet, with attention, I believe the following will be found generally sufficient.

Any one free from nasal obstruction, can breath freely through the nose, with the mouth closed. Having done this for a dozen successive respirations, let him inflate the cheeks, and keep them so by pressing the tongue against the roof of the mouth. In this manner he will immediately, or very soon, be able to breathe through the nose just as freely with the cheeks inflated, as in the ordinary state.

When he can do this, without effort, the large end of the blowpipe is to be taken between the lips, and the mouth inflated as before. The pressure

of the cheeks will thus force a stream of air through the pipe; and the mouth will quickly become nearly emptied.

It must then be inflated a second time, by forcing in air from the lungs, over the tongue, and again suffered to empty itself through the pipe, by the pressure of the cheeks; continuing the respiration all the while, at ease, through the nose. When this has been repeated several times, so as to have attained the power of breathing calmly, whilst the current through the blowpipe is produced entirely by the action of the cheeks; the next step is made by forcing in air over the tongue, *before* the mouth has completely thrown out the air it contains, and whilst the current is still continuing through the pipe. Thus he may keep up a continuous blast for a quarter of an hour, or more, without impeding his respiration, and with no other fatigue than of the lips where they press the pipe.

The next lesson is the regulation of the blast. For this purpose a candle with a longish snuff is to be lighted, and placed in a sloping position; the upper part of the wick being bent down nearly horizontal toward the same side as the candle leans. It is to be so placed that the operator, with both elbows leaning steadily on the table, can apply the point of the blowpipe just behind the flame, so as to blow it along the bent wick.

The blowpipe should not quite touch the flame, and should direct it a little clear of the wick, that the jet may be round and smooth. When properly produced, it will consist of a clear blue-pointed flame inside, with a yellow, or reddish, transparent outer one. If the outer flame is bright, dense, and sooty, like the ordinary flame of a candle, it indicates that the blast must be stronger, or that the aperture of the blowpipe must be enlarged, or brought closer to the flame; unless a smaller candle be employed.

Now, supposing the operator to have produced a clear, smooth flame, which does not smoke the edge of a sixpence placed at the blue point within it, let him rest the bend of the blowpipe, as well as his elbow, so that the instrument may remain perfectly steady.

He must now practise until, by regulating the pressure of his cheeks, and the filling of his mouth from the lungs, he can keep the jet, without puffs and jerks, as constant and firm as the pipe itself.

Let him next make a little cavity (two or three times the size of a split pea) in a piece of charcoal, and putting therein a fragment of malachite (green copper) not much larger than a mustard-seed, bring it up to the flame, so that the inner blue point shall act directly upon it: in a few seconds he will find it reduced to a bead of malleable metallic copper.

Continuing the blast, let him remove it into the outer flame, almost to the extreme point: he will soon find it covered with a crust; and, if put on one of the pipe-clay squares, it will be calcined throughout by the action of the external flame, and crumble under the blow of the hammer. Let him treat in the same manner a fragment of carbonate of lead: he will find it reduced still more readily in the blue flame, and calcined with almost as much ease in the external one.

Hence he will learn the third lesson, that the flame answers the double purpose of calcining and reducing furnaces; the outer flame being the calciner, the inner one the reducer. And this is the most important point of blowpipe knowledge.

The reducing action is increased by removing the blowpipe a little farther from the wick, so as to give the external flame more density, and a slight tendency to smoke the assay. The calcining action, on the contrary, is promoted by putting the point of the pipe quite into the flame of the candle,

so as to make the outer flame of the jet very transparent, and only just visible. A little harder blowing is suitable for calcination than for reduction.

Most of the ores having a metallic aspect (as yellow and grey copper, lead ore, mundic, white cobalt, &c.) contain sulphur, and require much calcination before being reduced: otherwise, the lead may be deficient in quality or quantity, or in both. Some of them fly on being heated, in which case the flame must be brought upon them gradually, letting it first pass over them, and raising them very slowly into it.

But there are some which decrepitate and fly about, when heated even in this cautious manner. With these it is necessary to use a second piece of charcoal as a cover. For this purpose, taking two pieces, each having a flat side, two inches long, let a groove be cut along one of them about a quarter of an inch wide and deep, and a cavity of the usual size, in the middle of the other. Place in the cavity a fragment of assay, four or five times as large as that usually employed, and put the grooved piece over it, so that the flat sides shall coalesce, and the cavity fall under the groove. The blowpipe jet, being then sent along the groove, will soon cause the assay to decrepitate, whilst the greater part of the projected fragments will be caught in the groove.

As soon as the decrepitation has ceased, the jet should be discontinued, and the assay thrown out on a plate. A particle will then most likely be found, large enough to work upon; but if not, the operation may be repeated on a larger piece, or a good knob of it heated red, in the bowl of a tobacco-pipe, covered, in a common fire. After once being heated red, the assay is no more subject to decrepitation.

In calcining ores, care should be taken not to melt them, as that retards the process: the heat should be applied at first gently, keeping the assay beyond the reach of the flame, and approaching as the sulphur is driven off, and the assay becomes less fusible. This process should be performed on the pipe-clay cupells.

When the sulphur, &c., is entirely driven off (which is sometimes a good while after the smell has ceased,) the assay should be removed to the charcoal, and tried in the reducing flame; a little soda being added if reduction does not readily ensue. If it still refuse to yield a metallic globule, it may be cooled quickly by being dropped on a knife-blade, or into a silver spoon, and then taken up on a bit of wax, and tried with the magnetic needle. If, on the approaching, or touching, one of the ends of this instrument when mounted as above described, a strong attraction be manifest, the assay is probably iron, which may be farther proved as hereafter stated.

If it do not produce a bead, or if the bead produced be not easy to recognise by itself, or by the streak it produced on a black gun-flint, a minute portion of it is to be melted with a little borax, in the reducing flame, on a clean surface of the charcoal; taking the precaution to melt the borax first, and see that it produces a colourless, transparent bead. If it be either coloured or opaque, after good fusion, it is unfit for the purpose; and another must be made, at a different part of the charcoal. The borax will swell and twist (like burning leather) in melting, but is easily brought back to a bead, by a little management of the hand.

If, when a portion of the assay is melted in it, the borax remain colourless and transparent, more assay is to be added, and the addition continued until it manifest some colour, or can no longer be rendered transparent; or, if the colour be too dense to be distinguished, more borax must be used.

A portion of borax is then to be fused upon a pipe-clay cupell, over the surface of which it will spread like a glaze, and will impair its whiteness, but must exhibit no particular tint of colour: if it do, another piece must be employed.

A minute portion of the assay, or, rather, of the borax bead containing it on the charcoal, is now to be added, and well fused on it, in the reducing flame, increasing the dose of assay or of borax, as before, unless colour be manifested.

Some of the metals are volatile, and will give neither bead nor colour to borax, but evaporate before the blowpipe in a white smoke, often of a strong odour; sometimes covering the charcoal with a white powder, or depositing a circular halo upon it, white or coloured, at a little distance round the assay. This halo may be again driven farther, or dispelled, by the action of the flame; generally of the reducing flame, but sometimes of the other. By these characteristics the volatile metals may be distinguished, as effectually as if they left a bead.

It happens also, frequently, that a volatile metal is combined with one that is fixed; as in white mundic, composed of arsenic and iron. The blowpipe then separates them; the arsenic, driven off, is known by its smell, and the iron, remaining, by its action on the magnetic needle.

But when two or more volatile metals are united, or two or more of the fixed kind, they are less easy to discriminate, and require both practice and study.

The following rules may generally be relied on; but in cases where the assay gives ambiguous or complicated indications, proportionate caution is necessary both in operating and deciding.

The miner will commonly judge a mineral to be metallic, if it has a metallic aspect, not destroyed by scraping; or if it feels particularly heavy in the hand. But others also discover metallic properties to the blow-pipe, by

1. Yielding a bed of metal, to the reducing flame, on charcoal; which is facilitated by the addition of soda.
2. Passing off in vapour, more or less dense.
3. Attracting the magnetic needle, after heating in the reducing flame, on charcoal.
4. Colouring borax strongly, either on charcoal or pipe-clay.

(TO BE CONTINUED.)

The Preparation of China Clay.

"An intelligent foreigner says, in traveling from Paris to St. Petersburg, from Amsterdam to the furthest part of Sweden, and from Dunkirk to the extremity of the south of France, one is served at every Inn, upon English (earthen) ware. Spain, Portugal, and Italy, are supplied with it; and vessels are loaded with it for the East Indies, the West Indies, and the continent of America." * * * "The manufacturing part alone in the potteries, and their immediate vicinity, gives bread to 15 or 20,000 people, yet this is but a small object, when compared with the many others which depend on it."—*Lardner's Cyclopædia*, xxvi., 16-20.

Mr. Barlow in the "Encyclopædia Metropolitana" states that the Cornish china-clay forms from eight to twenty-eight per cent., of *body* of porcelain, and the china-stone from four to eight per cent., whilst in the *glaze* from twenty-three to forty per cent. of the stone is employed.

Dr. Boase says that the quantity of china-clay exported from the neighbourhood of St. Austel "exceeds 7000 tons per annum: and when to this

is added the amount of china-stone, which is about 5000 tons more, the whole forms a very important branch of commerce. In estimating the value of these works to the community, it must also be remembered that they are situated in bleak barren moors; thus affording employment and support to several hundred persons, where the impracticable soil would hardly maintain a single family."—*Cornwall Geol. Trans.* iv. 236-7.

There are three districts in this county in which the china clay and china-stone, occur, viz., in the parishes of St. Austel, St. Mewan, St. Stephens, St. Dennis, and Roche, all near St. Austel: in the parishes of Breage, and Germoe, near Helston: and in the parish of Towednack, near Penzance. The two latter are, however, quite insignificant, when compared with the first named.

"The china-stone is a kind of granite, the felspar of which has undergone a partial decomposition: it is carefully selected, so as to be entirely free from schorl, and requires no other preparation for the market than to be broken into a size convenient for carriage. This granite is of a peculiar nature; it does not contain any mica, but numerous glossy scales of greenish yellow talc. * * * These beds of porcelainous granite are very numerous. I could not ascertain their bearings in every case: they were, however, very commonly from north to south, or rather east of north, and west of south." —*Boase. Cornwall Geol. Trans.* iv. 236.

In all granite districts, observes Dr. Turner, "Phil. Mag." (1833, iii. 22.) the formation of porcelain clay from the rocks, rich in felspar, exemplifies in a striking manner the affinities of water and carbonic acid, for potash and soda, and that of oxygen, for iron.

"It is probable that the long continued action of pure water might produce decomposition; but the effect of its affinity for the alkalies of the rock is materially aided by that of carbonic acid for the same basis. This is shown by the increased decomposing power of water, when charged with carbonic acid, and by the action of moist carbonic acid gas on granite."

It has been seen by the foregoing extracts, that the prevailing opinion is that china clay is derived from the decomposition of the felspar in a particular kind of granite; our object will now be to describe the operations pursued for the separation of the clay from the other constituent minerals of the rock, with which it is found mixed.

The vegetable mould and rubbish (meat-earth, and overburden) being removed, several pits, or tanks, are prepared and rendered water-tight: they are so placed that a communication may at pleasure be made, or stopped, between almost every one of them.

The first two or three of these are about ten or twelve feet in length, and three or four in breadth and depth; the others are much larger, say from thirty to forty feet long,—fifteen to twenty feet wide, but of much the same depth as the first.

A stream of water is now introduced at the highest part of the spot intended to be worked, and a fall of a few feet is preferred, in order to facilitate its action on the friable material; which is not unlike mortar in appearance, and contains abundance of siliceous gravel; much as quartz is disseminated in the parent granite. The action of the water on the clay is assisted by the workmen, who agitate it with shovels, pick-axes, and other tools; and it is thus carried, in suspension, into a furrow, through which it is conveyed into the pits or tanks. Much of the silicious and other ponderous matter subsides in the channel, and in the first pit; whilst substances of smaller gravity are carried into the adjoining ones (the *mica-pits*,) and the fine

clay, being more completely suspended in the water, is carried into the larger receptacle, where it gradually subsides, and the colourless liquid is permitted to escape through apertures prepared for the purpose, at small heights above each other, so as to allow of its draining off, without disturbing the sediment of clay.

In tempestuous weather, however, the subsidence of the clay occupies a very much longer time than ordinary, and artificial means are employed for effecting it. A solution of alum is sprinkled on the surface of the turbid liquid, from a common garden watering-pot, which occasions the clay to fall down very rapidly.

When a sufficiently thick deposite of clay has been thus obtained in the tank, as much as possible of the supernatant liquid is allowed to run off, and the clay is removed to another place, where being more thinly spread, evaporation is facilitated. This is accomplished by means of a common lifting pump, of small size, which is introduced into the first receptacle, and empties itself into the second. The mud, however, is so thick, that it will not readily flow towards the pump, which is scraped by a toothless wooden rake; and *even after it has been raised by the pump, it is so dense as to require being raked therefrom, and into the second pit.*

When the moisture has evaporated sufficiently to bring it to a proper state of consistence, it is cut into rectangular lumps, of from six to twelve inches square, which are (as the season may be,) removed either to the adjacent plain or into the drying-house, which is a shed open at the sides, and contains frames on which the clay is so placed as to permit a free circulation of air. When dry, the clay is scraped free from all impurities, and packed in cases for the market.

The best qualities are perfectly white, but the inferior have some slight tinges of yellow; all are, however, free from the smallest traces of grit.*

It is exceedingly curious, that those who are conversant with the preparation of china-clay, will readily ascertain the position which each end of a given lump occupied with respect to that of the pump, by which the mud was drawn from one tank into another. Because, when scraped with any sharp instrument in a direction contrary to that which it flowed, a smooth surface cannot be obtained; whilst, when the direction of the current and of the scraping are coincident, it readily presents an even surface.

There is something very interesting in this fact, which, if properly understood, might perhaps throw some light on the bedding and lamination of rocks, and other intricate problems of geology.

Much has been said on the *causes* of the decomposition of the felspar in the granites (protogenes,) which contain talc instead of mica, but there seems good reason for the opinion of Dr. Boase ("Phil. Magazine" for the present month,) that it is owing to some peculiarity in the composition of the felspar itself. This excellent analyst has examined some of the china-clays of Cornwall, and finds

	St. Stephens.	Breage.
Silica,	39.55	40.15
Alumina,	38.05	36.20
Magnesia,	1.45	1.75
Water,	12.50	11.66
Insoluble residue, quartz and talc,	8.70	9.50
	100.25	99.25

* The drying pits are covered with the refuse quartzose sand, previously to being used, in order to prevent the clay from adhering to the bottom.

The inquirer may find much instructive matter in all the authorities we have quoted; and as well in Berzelius, *Traite de Chimie*; Becquerel, *Traité de l'Electricite*; and Berthier, *Annales de Chimie*, lxii. 225. Mining Journal.

Memoir on an apparatus for evaporating and concentrating Saccharine Juices at a low temperature. By M. DEGRAND, Civil Engineer at Marseilles.

The author of this memoir obtained the prize of 4000 francs offered by the "Society of Encouragement," for the construction of an apparatus that would evaporate saccharine solutions under a pressure of two-tenths of an atmosphere. The memoir is contained in the Bulletin of the Society, for July 1836. Two large plates, descriptive of the apparatus, accompany it, but they are executed in too elaborate a manner for the purpose of this Journal. We shall therefore endeavour, by a verbal description, as perspicuous as we can render it, to furnish our readers with an intelligible account of this ingenious scheme for performing evaporation under reduced pressure.

The boiler is of a globular form (though not necessarily confined to this figure) consisting of two hemispheres, fastened together when in action by flanches and screws in the usual way. The lower hemisphere is lined with the coils of a worm for the admission of steam from a generator, the heat of which carries on the evaporation. This worm (or there may be more if needful) is of course immersed in the fluid to be evaporated. The hemisphere has likewise a double, or false, bottom, leaving a space into which the steam also enters.

When the boiler is ready to receive the juice, it is to be exhausted of air either by the action of an air pump, or in any other convenient way. One of the methods described in the memoir, and which appears to be the most eligible, is to fill the boiler with steam from the generator, allowing the air to escape through a cock, all the other passages being closed. The stopper of a tube proceeding from the upper half of the boiler, and terminating in a cistern containing the juice to be evaporated, is then opened, and as the boiler cools, the pressure of the atmosphere forces the juice into the boiler until a float within it, indicates the requisite quantity. The boiler contains two sight holes, or windows, formed of thick glass, through which the internal operations can be observed. When the ebullition becomes too rapid, a small quantity of oil is admitted from a cup placed over the boiler. Steam of the temperature of about 112° Cent. = 234° Fahr. being then admitted into the worm and the false bottom, ebullition soon commences and the evaporation rapidly proceeds.

The vapour from the juice passes into a tube which proceeds vertically from the top of the boiler to some distance, and then turns at right angles and enters horizontally into the upper opening of a long worm, which is placed erect within a vertical cylinder which encloses it on the sides, but which is open at top and bottom, serving, of course, as a kind of chimney through which, while the worm is hot, there is a continually ascending current of air.

The horizontal position of the tube which conducts the vapour from the boiler to the refrigerating worm, is surrounded by a metallic sleeve, or muff, for receiving the water, which by falling on the outside of the worm in a continual shower is to condense the internal vapour. This sleeve is of course joined to the tube in an air-tight manner. The water is admitted

into the cavity of the sleeve through a tube from a reservoir placed above it. This water soon acquires the temperature of the horizontal tube itself. It passes from the sleeve through a separate tube, and is discharged into a circular sieve situated immediately over the refrigerating worm, and falls in a fine shower upon the worm, trickling down upon it to the bottom. The ascending current of air through the open cylinder which surrounds the worm, carries off the external vapour and promotes the refrigeration. The rapidity of this current may be accelerated by increasing the height of the cylinder, or by connecting its top with a neighbouring chimney, or by a ventilating drum, or by a fire at the bottom. To produce a maximum of effect in the refrigerator, the ascending current of air must be at a maximum, and as this velocity is dependent on the excess of temperature of the aqueous shower, and the coils of the worm, over that of the external atmosphere, it follows that the temperature of the descending water ought, at its source, to be at a maximum. Now, this can be the case only when the source of the shower is at the same temperature as the vapour to be condensed. If less hot, it will draw in less air, if hotter it would raise the temperature of the vapour to be condensed. Consequently (says the inventor) the water employed in the cylinder, as the agent of refrigeration, produces a maximum of effect when it is as warm as the vapours it is destined to condense.

If (he adds) this simple proposition be announced,—"that in refrigerating with water, the less of it will serve when it is warm than when it is cold; and that there is the least expenditure of water when it is as hot as the vapour on which it is to operate,—this proposition would appear very singular; and yet, it is conformable to the laws of nature and appears strange only, because, until the present time, cold water only has been employed in refrigeration."

It is for this reason that the warm water which is pumped into the reservoir is passed into the sleeve, for the purpose of being heated as nearly as possible to the temperature of the vapour, before it is poured into the sieve and thinly distributed over the worm. That which escapes evaporation, in trickling over the worm, is collected at the bottom of the cylinder, and is pumped back, while still tepid, into the reservoir.

It would thus appear that the evaporation of the hot water which is distributed over the worm, aided by the current of air through the cylinder, carries off more heat, and condenses the internal vapour more rapidly, than would be effected by cold water; and that this evaporation and condensation are so effectual as to sustain the ebullition of the sirup under the reduced pressure required by the conditions of the premium. The apparatus being exhausted of air in the commencement, and no ærated water being allowed to enter, it is sufficient (says the memoir) in order to evaporate the saccharine solution under a tension of vapour equal or inferior to 0.2 of atmospheric pressure, that the refrigerating surface of the worm and the mass of air which is drawn into the cylinder, be in harmony with this effect.

When the sirup is sufficiently reduced, the boiler is evacuated without allowing the air to enter. This is effected by expelling the air from the vessel into which the reduced sirup is to be emptied; when on opening the communication the sirup runs into this vessel, the passages are cleared of air, a fresh quantity of juice is admitted into the boiler, as before, and the process is continued, and thus a greater number of runnings can be performed in a given time.

The mode of sugar boiling above described is considered by the inventor to be a great improvement on the steam process of Howard. It may be characterized, in contradistinction to all other kinds of apparatus for the evaporation of saccharine solutions, by the brief remark,—*that it refrigerates with hot water, and expends only a minimum of water.*

This new process has received the sanction of experience, and is successfully conducted on a large scale in various refineries.

Instead of effecting the condensation with water, the idea occured to *Charles Derosne*, to employ the clarified juice itself. This is distributed over the cylinder by means of the sieve, as before described; and, without increasing the fuel employed, it economises the process to the amount of 40 per cent. G.

On some Silicates of Alumina. By R. D. Thomson, M. D.

To the compounds of silica and alumina great interest is attached, in consequence of the frequency of their occurrence, the diversity of their external characters, even when closely allied by chemical composition, and their utility in manufactures.

Notwithstanding the care which Berthier has bestowed on the subject, the arrangement of this class of minerals labours under great disadvantages, and it is questionable whether our present knowledge be adequate to enable us to introduce that precision which has been applied to other bodies possessing analogy of chemical composition.

I was led to the investigation of the composition of these simple compounds, from having lately met with three species of minerals, which appeared to possess distinctive external characters, but could not be referred by these features, with certainty, to any described species. They were all found in rocks connected with a red sand-stone, which is deposited in horizontal strata along the banks of the Tweed, in the neighbourhood of Melrose, and appears to be referable to the old, red, sand-stone series, or transition formation, being occasionally interrupted by dykes of green-stone and clay-stone porphyry. In the latter of these trap rocks, two of the minerals alluded to are found in considerable quantity, while the third, seems to occupy a place in the sand-stone itself.

I shall describe their characters and composition, and compare them with the simple compounds of silica and alumina which have been examined. The first of these minerals, I have termed *Tuésite*,* from Tuésis the river Tweed. It occurs in veins in porphyry, or indurated sand-stone slate, which is intimately connected with the old, red, sand-stone. Its colour is milk white; opaque; lustre, dull; sectile. Hardness 2.5. Specific gravity from 2.434 to 2.558.

Before the blow-pipe *per se*, it becomes blue and brittle, fusing with carbonate of soda into an opaque bead, and with borax and salt of phosphorus into a transparent glass. It forms an excellent slate pencil. A portion of the mineral was finely pulverized and fused with carbonate of soda. The silica being separated in the usual manner, the alumina was precipitated by caustic ammonia in the form of beautiful white flocks, which after determining its weight, was dissolved in sulphuric acid, with the addition of potash. Regular crystals of potash, sulphate of alumina, were the result of the gradual evaporation of the solution. The liquid remaining after separating the alumina was precipitated by oxalate of ammonia. The product

* See description, Thomson's Mineralogy, vol. i. 214.

was a small quantity of lime. The residual liquor was evaporated to dryness, the dry salts heated to redness, dissolved in pure water and boiled with carbonate of soda; a precipitate of magnesia ensued. This precipitate being weighed, was dissolved in dilute sulphuric acid. The whole of it dissolved, with the exception of a minute portion of silica, scarcely appreciable, and which produced an amber coloured bead when fused by the blowpipe with carbonate of soda.* The water in one trial amounted to 13.5 per cent., in another to 13.2.

The constituents are

Silica,	44.300	4 atoms
Alumina,	40.400	3 „
Lime,	0.755	
Magnesia,	0.500	
Water,	13.500	2 „
	99.455	

The formula, to represent its composition, which I am disposed to adopt, is

$$2\ Al\ S + Al\ S^2 + 2\ Aq.$$

Accordin to Berthier's view it would be,

$$2\ Al\ S^2 + Al\ Aq.$$

If, however, the lime and magnesia were taken into account, and supposing them to re-place alumina, we should have, according to the formula of Professor Thomson,

$$3\ Al\ S + 2\ Al\ S^3 + 3\ Aq.$$

Corresponding with this composition, we find several analyses; especially one by Boussingault, of a Halloysite, as he terms it, from Guatequa in New Granada, found in carbonaceous schist with anthracite, of a soft consistence, with a cheesy fracture, becoming transparent in water; and two of Kaolin by M. Berthier.

	Halloysite from Guatequa†	Kaolin. St. Yriex.‡	Kaolin. Schneeberg.§	
Silica,	45	46.8	43.6	= 4 atoms.
Alumina,	40.2	37.3	37.7	3 "
Potash,		2.5		
Peroxide of iron,			1.5	
Water,	14.8	13.	12.6	2 "
	100.0	99.6	95.4	

It is quite obvious that these correspond with the formula which we have already given. The halloysites of Berthier differ essentially in their external character from tuesite. The specimen from Anglar, near Leige, possesses a density of 1.8 to 2.00, or, according to Ingelspach Lariviere, from 1.82 to 2.09.‖ Fracture compact, waxy, conchoidal, pure white, blueish, scratched by the nail, taking a polish under the finger. It is found in veins of hydrate of iron mixed with galena, carbonate of lead and calamine, which

* This is a peculiar state of silica well known to chemists as occurring in the latter stage of analyses, which has frequently been mistaken for Titantic acid.

† Ann. de Chim., liii. 439.

‡ Traité des Essais par la Voie Seche par M. Berthier, i. 58. § Ib.

‖ Ann. des Mines, v. 310.

traverse transition limestones, in masses varying from the size of the fist to that of a cubic metre. Two specimens afforded,*

	Anglar.	Hall.	
Silica,	39	39	4 atoms.
Alumina,	34	35	3 "
Water,	26	25.5	5 "
	99	99.5	

Their formula is,

$$2\,Al\,S + Al\,S^3 + 5\,Aq.$$

If then the lime and magnesia be considered foreign to the composition of tuesite, we shall have halloysite forming with it a sub-species of the same mineral, but possessing decidedly distinct characters, apparently occasioned by the addition of 3 atoms of water. And it seems necessary that the term halloysite should be restricted to those compounds consisting of silica and alumina, in the proportion of 4 atoms to 3, with 5 atoms of water, while tuesite contains the same proportions of solids but possesses only 2 atoms of water.

2. *Bisilicate of Alumina or Fuller's Earth.*—This mineral is found in round masses in the bed of a stream, associated with clay-stone porphyry, near Maxton. Sp. Grav. 2.394.

With nitre, soda and salt of phosphorus, fuses before the blowpipe into an opaque mass. With borax, fuses into a transparent bead, pale amber coloured when hot, colourless when cold. Colour yellowish white; fracture earthy, soft, soiling the fingers; scratched by the nail, tuésite, and sulphate of lime; adheres to the tongue like halloysite; contains crystals of decomposing felspar interspersed through the mass. Its constituents I ascertained to be,

Silica,	57.105	4 atoms.
Alumina,	31.850	2 "
Magnesia,	2.615	
Water,	7.280	1 "
	98.850	

and its formula $2\,Al\,S^3 + Aq.$

Berthier has included a mineral possessing exactly the same composition, with less water, under halloysite, and another under kaolin, as is exhibited in the following table:†

	Halloysite. Fahlun.	Kaolin. Normandy.	
Silica,	46.8	50	2 atoms.
Alumina,	26.7	28	1 "
Peroxide of iron,	5.0	5.5	
Magnesia,	0.4	.7	
Lime,	3.0	5.5	
Water,	13.5	9.5	1 "
Potash,	.	2.2	
	95.4	101.4	

Berthier considers these two specimens as affording instances of felspar in different states of decomposition, and although he states no circumstances which tend to establish his supposition, yet it is possible his conjecture may

* Traite des Essais, i. 58. † Traite des Essais, i. 60.

be correct. I could observe no fact, however, which could give the slightest countenance to the idea, that Tuésite or Fuller's earth, but more especially the former, is in any way connected with felspar.

3. In the sand-stone, which has been already described, a soft, whitish substance occurs. It is smooth, yielding to the finger, containing greenish streaks, and answers to the description of lithomarge, or rock marrow. The specific gravity is 2.457. Its constituents are, according to my analysis,

Silica,	56.850	19.0 atoms.
Alumina,	25.000	7.5
Potash,	6.178	1.
Lime,	3.492	.5
Magnesia,	2.640	.5
Water,	5.840	4.
	99.900	

Its composition, including the lime and magnesia with the alumina, is expressed by the formula,

$$8\tfrac{1}{2}\ \mathrm{Al\,S^3} + \mathrm{K\,S^3} + 4\ \mathrm{Aq.}$$

Berthier analyzed two Kaolins which approach lithomarge in composition, magnesia taking the place of the potash.

Kaolin from St. Tropez, $3\ \mathrm{Al\,S^2} + \mathrm{M\,S^3} + 2\ \mathrm{Aq.}$

Kaolin from Mende, $4\ \mathrm{Al\,S^2} + \mathrm{M\,S^3}$.

For the sake of greater precision, I shall give a view of the composition of the various Halloysites and Kaolins analyzed by Berthier and Boussingault, including along with them the minerals analyzed by myself, and expressing their constitution by formula.

I. 1. Tuésite, $2\ \mathrm{Al\,S} + \mathrm{Al\,S^3} + 2\ \mathrm{Aq.}$

Under this species are comprehended Halloysite from Guatequa analyzed by Boussingalt, and two Kaolins from St. Yriex and Schneeberg, examined by Berthier, and Clay of Angleur.

2. Halloysite, $2\ \mathrm{Al\,S} + \mathrm{Al\,S^2} + 5\ \mathrm{Aq}$,

including the Halloysites of Anglar and Hall, and perhaps, Nontron.

II. Bi-silicate of Alumina or Fuller's earth,

$$2\ \mathrm{Al\,S^2} + \mathrm{Aq.\ and\ Al\,S^2} + \mathrm{Aq},$$

expressing the composition of Halloysite from Fahlun, Kaolin from Normandy, and Fuller's earth from the Tweed. Kaolin from Meissen possesses less water $= 3\ \mathrm{Al\,S^2} + 2\ \mathrm{Aq.}$

III. Linzinite is the name which John originally gave to the Halloysite of Hall. It may, with propriety, be applied to the ter-silicates of alumina, whose composition is denoted by

$$\mathrm{Al\,S^3} + 3\tfrac{1}{4}\ \mathrm{Aq.}$$

Berthier analyzed two Halloysites from St. Sever and Confoleus possessing this constitution, besides clays from Hoeganas, Forges, Montereau, Cymolite, Cologne, with less water.

IV. Lithomarge, $8\tfrac{1}{2}\ \mathrm{Al\,S^3} + \mathrm{K\,S^3} + 4\ \mathrm{Aq.}$

Under this species, we may, perhaps, include the Kaolins from St. Tropez and Mende whose expressions have been already exhibited.

In presenting the preceding arrangement, it is only my intention to assist in simplifying the study of these interesting compounds; for it appears more beneficial to classify minerals, not according to theoretical views, but according to their actual nature. M. Berthier, whose analytical accuracy none will dispute, has thrown all the simple silicates of alumina into one class: but, it may be asked, are none of these compounds as much true

species as other minerals with which we are acquainted? or, if they are different states of decomposing felspar, are they not in this respect analogous with many species, which have been presumed, with considerable probability, to be the products of decomposing rocks? Record General Science.

Sugar:—Examined in its Commercial and Chemical Relations. By George Guynne and James Young.

Before detailing the results of our experiments on sugar in its relations to chemical agents, it may be advisable to give a brief outline of "sugar refining," as it is now practised in Great Britain.

The various modes of sugar refining may be comprehended under two general divisions. First, where the raw material is clarified by blood and, perhaps, a little animal charcoal in fine powder, the water evaporated in a pan over the naked fire, and the remainder of the process of *whitening* performed by pouring over the *moulds* containing the sugar, a very thin paste of aluminous clay and water. This method is generally distinguished by the name of the *old plan.*

Second, where the raw material is in a great measure freed from impurities by being made into *meltings*, the meltings dissolved in water by the aid of steam instead of the naked fire; the evaporation performed in vacuo, and the remainder of the process of whitening performed by pouring over the moulds containing the sugar, a sirup, technically called *liquor*, made from refined sugar. This method is generally known by the name of the "patent process," or Howard's plan.

1. *Old Plan.*—Although the old plan is decidedly inferior to Howard's, in economy and speedy production of the refined material, it is still carried on to a considerable extent in London. The proprietors of such sugar houses are, with perhaps scarcely an exception, Germans, or the descendants of Germans, and much wedded to old habits. Although they have the experience of above twenty years before them of the successful working of Howard's plan, they still think their own method the better of the two, and expect to live to see the total failure of the other.

In the working of an "old plan" house, the raw sugar is mixed over night with lime and blood, in the proportion of about four gallons of the former to one of the latter for each hundred weight of sugar; next morning the fire is kindled under the copper, and by careful management, when coming to the boiling point, the insoluble impurities are entangled in the coagulative albumen of the blood and carefully skimmed off. This operation is called *clarifying*, and the mass of blood and impurities *scum*; when as much scum has been removed as possible, the sirup is filtered through a blanket, distended over a basket, to separate the flocculent matter which has escaped the *scumming ladle.* The "scum" is worked over again with water, in order to get as much matter out as possible. The sirup is now evaporated over the fire until it reaches the proof point. *Proof* is a technical phrase employed in the sugar-house, to denote that point in the evaporation of water from sugar, which will afford the greatest weight of crystals *(grain.)* consistent with a due draining of the sirups *(mother liquors.)* from the sugar, when put into moulds. Proof varies according to the quality of the sugar; where the sugar is of an inferior kind, it requires to be higher *(stiffer proof,)* than when of a better description. Although the thermometer is far more

accurate, as an indicator of proof, than the *finger and thumb*, still few of the German boilers employ it; they look on it as new fangled toy, not to be depended on. Finger and thumb proof is performed by taking some of the boiling sirup from the copper with a small ladle, and putting it between the fore-finger and thumb, and then separating them, so as to form a thread; by the breaking of this thread the proof is taken. When a thermometer is employed, it is taken by single inspection of the scale, and it varies from about 232° Fah. to 248° or 250°, according to the quality of the materials; 232° would be *proof* of double refined loaves, and 248° the proof of the lowest quality of sugar "bastards;" when this takes place, the boiling fluid is laded into shallow copper vessels, called "coolers," where it is occasionally stirred, in order to promote the formation of the sugar-house crystals. After remaining the requisite time in the coolers, it is transferred into "moulds;" these are conical shaped vessels (usually unglazed earthenware, though iron is preferable,) open at top, with a small hole at the apex; the hole is stopped with a piece of paper, to prevent unconsolidated sugar from flowing out; next morning the paper plug is withdrawn, and the moulds placed in earthen ware, or iron, jars (pots.) In twenty-four hours about one-third of the weight of the contents of each mould will have passed into the pots in the state of *green sirups*. When the green sirups have properly drained, the upper surface of the sugar is scraped off to perhaps the depth of an inch (*brushing off*;) the "brushings off" are mixed with water into the consistence of paste and poured on the moulds. Some hours after this, a cream of aluminous clay and water is poured on the sugar; the water slowly parts from the clay, percolating through the sugar, a portion of which it dissolves, carrying with it the colouring matter and other impurities. This *claying* is repeated until the sugar is properly whitened. The sirups, which the claying forces out from the sugar, are distinguished by the names of *second sirups* and *drops*. The "green sirups" are evaporated separately and made an inferior sort of sugar *(bastards;)* the liquors which drain from the bastards constitute treacle, an article from which (although very sweet to the taste) no further crystals have as yet been produced. The "seconds sirups" and "drops" are used along with fresh material in making refined sugar.

The sugar, after draining completely on the pots, is turned with the face downwards for twenty-four hours, and then papered. It is now put into the drying stove (heated to about 135° or 140° Fah.) where, in a few days, the water it contained is entirely evaporated away, and it is now ready for sale.

The entire period which elapses from the time the sugar is "clarified" until taken from the stone averages, about forty days.

2. *Howard's Plan.*—The leading features of this plan are the substitution of steam, as a heating medium, instead of the naked fire, and the evaporation of the liquor performed in vacuo. Where the patent process is strictly pursued, the following are the different steps:—The "raw sugar" is put into a vessel with a "jacket" heated by steam (*melting pan.*) and some water added and well stirred; when the water reaches about 180°, it is transferred into large moulds where it is allowed to consolidate; the stoppers of the moulds are then withdrawn, and the green sirups separate from the sugar, leaving it comparatively pure. The sugar is now called *meltings*, which are put into a vessel heated by steam (*blow ups.*) and lime water, together with a little alumina (finings) added; after boiling for a few minutes the liquor is let into the filter to separate insoluble matters. When

the liquor becomes *bright* it is transferred into the vacuum pan, where the necessary degree of evaporation takes place. This operation is examined from time to time by means of a brass rod (*proof stick*,) with a small hole near its end; by turning the proof-stick, a cock is opened in the vacuum pan and some of the sugar brought away in the small hole. Crystals begin to appear at an early stage of the evaporation, and gradually increase in size until the full charge is in the pan. In the "old plan" no crystals show themselves until the "liquor" is laded into the "cooler," and the temperature has fallen to about 165°. When the requisite degree of concentration* has taken place, a valve is opened in the bottom of the pan, which allows the fluid sugar to escape into a copper vessel (*heater*,) heated by a steam jacket placed underneath. In the "heater" it is constantly stirred until the temperature rises to about 175°, when it is transferred into moulds, and next morning carried into the "curing floors," when the stoppers are withdrawn and the moulds placed in "pots," in order to allow the green sirups to drain.

The subsequent treatment is similar to that of the old plan, with the exception that instead of "claying," there is poured over the sugar a saturated sirup (*liquor*) made from refined sugar itself. This "liquor" whitens the sugar in the same manner as claying; and possesses the advantage of dissolving no part of the crystals, besides greatly economising the time. Instead of taking forty days to produce refined sugar, it is here produced ready for sale in about twenty days.

The green sirups produced from the meltings are made into bastard sugar; the green sirups from these constitute treacle. The green sirups which the refined sugar yields are made into an inferior sort of refined sugar (*brown lumps*,) and their green sirups make an inferior sort of bastard sugar. The "*second sirups*" and "*drops*" are worked in a similar manner to that described in the old plan.

Although more than twenty years have elapsed from the publication of Mr. Howard's plans, the only real improvement made since his time is a method of percolating the *raw* sugar liquor through an immense body of animal charcoal, in the state of coarse grains. This plan is now extensively employed in several refineries in this country, and supersedes the making of meltings, besides shortening the period of producing the refined sugar three or four days. The following is a brief description of the process as practised in one of the largest sugar-houses in London.

The "raw sugar" is dissolved in the "blow up" in the usual manner; a little finings and a very small portion of blood are employed, which materially assist the filtration; after boiling a few minutes, the liquor is filtered through *Schroder's bags*. From the *clear liquor* cistern the fluid passes into another apartment in which are several immense vessels, made of wood and lined with lead, nearly full of animal charcoal of the size of coarse gunpowder. The vessels have each a false bottom covered with cloth, and the charcoal is placed over the cloth to the depth of about thirty-four inches. The *clear*, but dark, coloured liquor is allowed to percolate slowly through the beds of charcoal. The first part is generally a little turbid from the presence of minute particles of charcoal; this turbidity soon ceases, and the liquor is then allowed to flow into the receiving cistern from whence it is transferred into the vacuum pans. If the charcoal be

* When the air-pump and other parts of the machinery are in good order, the temperature of the "liquor" in the vacuum pan at the highest range is about 160°; and the vacuum produced equal to support a column of mercury 26 inches high.

good, the liquor which flows through it is for some time as colourless as pure water, and perfectly free from every impurity possible to be removed.

It soon however becomes coloured, and in about seventy-two hours from commencing, no more liquor is allowed to flow on the "beds of coal." The liquor absorbed by the charcoal is obtained by repeated percolations of cold water.

The refinery above alluded to, works about 60 tons of sugar per week, and to carry on this work about 50 tons of charcoal are required. The charcoal, when it becomes exhausted of its decolorating properties, is again rendered *active* by being heated to dull redness in closed cylinders; this *restoring* costs about sixpence per cwt. of charcoal; and the quantity employed to decolorate a given quantity of "raw sugar" is at the rate of one cwt. to three cwt. of sugar.

The advantages this plan possesses over the old method of using charcoal in fine powder are—increased decolorating power and superior crystallisation. The disadvantages are,—the great quantity of charcoal required; the immense quantity of *sweet water* (thin solution of sugar) produced. This latter is indeed the principal objection against the plan, there being a great deal more "sweet water" made than can be got rid of in any other way than by evaporating it separately. Another disadvantage attending the "coarse grained" charcoal is the liability of spontaneous acidity taking place in the liquor, long before the charcoal is exhausted of its decolorating property; when this event occurs, the refiner has no remedy but getting out the liquor as speedily as possible.

It is exceedingly difficult to state accurately the produce of 112 pounds of raw sugar, as the refiners are most determinately silent on this subject. There can however be no doubt but that, even by the old plan, a greater weight of refined sugar can be obtained than the Government have founded their calculations in allowing drawback duty. The following weights are not far from the truth:—

Old Plan.		*Howard's Plan.*	
Refined	70 lbs.	Refined	79 lbs.
Bastard	22	Bastard	17
Treacle	20—15½ lbs. solid matter.	Treacle	16—12 lbs. solid matter.
Waste	4½	Waste	4
	112		112

In all descriptions of saccharine matter which assume the crystalline form, there exists a certain quantity of crystals or (in the language of the sugar refiner) *grain*, which quantity cannot by any process be increased, but may be diminished by the admixture of various foreign bodies. Several of the proximate vegetable principles, such as mucilage, gluten, tannic acid (tannin,) &c., diminish the production of crystals; all the acids produce similar results, so do all the alkalies, alkaline earths, and all those salts which, although neutral in composition, exert an acid reaction on test paper.

If a filtered solution of "raw" sugar is submitted to proper chemical tests, it will be found to contain mucilage, gluten, tannic acid, acetate of lime, chloride of sodium (muriate of soda,) oxide of iron, and saccharate of lime. That these bodies are foreign may be proved by taking some refined loaf sugar, dissolving it in distilled water, and then testing. On examination it will be found that none of the above bodies can be detected. That these

bodies diminish the production of grain can be proved by two methods:—1st. If a solution of raw sugar be freed from them, its crystallising power will be found much increased. 2d. If a solution of refined sugar have these matters dissolved in it by artificial means, its crystallising power will be found materially injured.

All the foreign bodies, with the exception of acetate of lime and chloride of sodium, can be removed from "raw sugar." The salts formed by acetic acid and soda being soluble, all that could be done with these two bodies would be to change them into some other soluble compound; at the same time they do not injure the grain of the sugar, unless the quantity is enormously great. Hence, from the impossibility of making "grain" where it really does not exist, the utmost improvement which can be effected for "raw sugar" is to free it from these foreign bodies, which injure its graining power, leaving its natural grain at the same time uninjured.

We consider that the two following causes, hitherto entirely overlooked by practical, as by scientific, men, have been the grand obstacles to improvements in the manufacture:—1st. The affinity which sugar itself, when in solution, exerts on many chemical agents. 2d. The great specific gravity of the liquor which the refiner is obliged to employ; the sp. gr. being rarely under 1.280 at 60°, and frequently indicating a greater density.

The two following experiments will prove that great spec. grav. materially modifies chemical decomposition. 1st. Take a quantity of *liquor*, spec. grav. 1.280 at 60°, made from refined loaf sugar, and dissolve in it 20 grains of sulphate of protoxide of iron; now add and dissolve as much cream of lime, as would have perfectly decomposed the 20 grains of sulphate if water was the solvent. No precipitation takes place. 2d. If a quart of liquor made from similar sugar, but whose sp. gr. is only 1.020, had dissolved in it 20 grs. of the sulphate of protoxide of iron, and lime is now added in the same manner, immediate precipitation of sulphate of lime and protoxide of iron, ensue.

That sugar in solution is capable of modifying in a surprising manner well known chemical results, will be apparent from the following experiments:—1st. Let some liquor, sp. gr. 1.020, made from refined sugar, be boiled for a few minutes on a large excess of caustic lime, and then filtered; the liquor thus dissolving a large quantity of lime. Let oxalic acid be added to some of this caustic liquor, until it is slightly acid to the test paper; then filter, to separate insoluble oxalate of lime. If this acid liquor be again tested with oxalic acid, it gives a fresh precipitate. Hence liquor, even of the low sp. gr. of 1.020 can contain excess of lime, and excess of oxalic acid in the same solution, without acting on each other; yet oxalic acid is capable, if added in great excess, of precipitating the entire lime from a liquor of 1.300 sp. gr. 2d. If the foregoing experiment is reversed by adding oxalic acid in the first instance, and then attempting to neutralize by lime, it will be found that an excess of lime must be employed before the entire of the oxalic acid can be neutralised.

We have chosen oxalic acid and lime simply on account of the great affinity they manifest for each other, and the striking character they give to the experiments from the insoluble compounds formed. It will, however, be found, on examination, that all other acids, alkalies, or alkaline earths act on each other precisely in the same manner as above described, when *liquor* is the medium in which they are dissolved.

It may, perhaps, be as well to observe, that the foregoing remarks apply in the strictest manner to all salts, which when dissolved in *liquor* are at-

tempted to be decomposed by the introduction of any alkali, or alkaline earth. Let sulphate of oxide of zinc, sulphate of protoxide of iron, ter-sulphate of peroxide of iron, sulphate or acetate of alumina, &c., be dissolved in "liquor," and the alkali, or alkaline earth, then added, no precipitation takes place; an excess of alkali, &c., must be employed, and this excess frequently redissolves the base of the salt as fast as it is decomposed. This redissolving arises from the power saccharine matter and alkalies, &c., have of forming soluble compounds with some of the oxides of the second class of metals, when neither saccharine matter, nor the alkalies, &c. separately have any such power. For instance, protoxide of iron is totally insoluble in "liquor," if such liquor is quite free from either acid or alkali, &c. A mixture of lime and water has no power whatever in dissolving protoxide of iron, but if some *liquor* saturated with lime is allowed, even when quite cold, to percolate through a bed of iron rust in coarse grain, 12 inches in depth, such liquor will issue from the iron as black as ink and completely loaded with protoxide of iron.

The same result attends the employment of sulphate of oxide of zinc. This salt has the property of precipitating vegetable colouring matter when in combination with tannic acid, and the latter is an impurity found in raw sugar. But the sulphate will be found to occasion only a very slight decoloration of the liquor. If lime be now added to decompose the sulphate, an excess must be used before decomposition takes place. This excess redissolves the oxide of zinc as fast as it is liberated: for lime and sugar, when in solution, act towards oxide of zinc as they do to protoxide of iron.

In the great sp. gr., &c., modifying affinity of *liquor*, we can trace the source of the many difficulties that have attended attempts in sugar refining. An examination of the properties of the different foreign bodies which can be removed from raw sugar, together with due estimation of the two foregoing causes, point out in what class of bodies to seek for an agent, or agents, capable of improving (if such a thing be possible,) sugar refining as now practised in great Britain.

Before attempting such an examination we may state some facts, which we believe to be new and of some value, connected with that plan (percolation through animal charcoal in a granular form) of refining sugar, which experience has pointed out as the most improved yet known.

[TO BE CONTINUED.]

Preparation of pure Antimony exempt from Arsenic and Iron. By JUSTUS LIEBIG.

The process proposed by M. Liebig is not expensive and may be executed with great facility, and it appears to have the advantage of removing at a single operation, arsenic, iron, and copper: only, if the regulus contain lead, it must be separated by other means. Antimony should be therefore taken free from lead; but if that cannot be procured, the latter metal must be first extracted from the sulphuret of antimony. For this purpose the method of Berthier is preferable: viz. melt together

100 Sulphuret of Antimony,
42 Iron,
10 Sulphate of Soda, dry, and
2 Charcoal.

From this we obtain from 60 to 62 of the regulus. This operation is easily

performed without leveling. The scoriæ is very fluid, and the hammer easily separates it from the metal.

The following is Liebig's process:—16 parts of the commercial regulus of antimony are coarsely powdered and mixed with one part of sulphuret of antimony and two parts of dry carbonate of soda; melt the mixture in a Hessian crucible, and keep it an hour in fusion; let it cool, break the crucible and separate the scoriæ from the metal. Pound up the latter coarsely, mix it with one and a half parts of dry carbonate of soda, and keep it again an hour in fusion. Treat the metal a third time in the same manner with one part of carbonate of soda. The dross of the first melting is of a deep brown, that of the second is of a clear brown, of the third a clear, or almost of a citron, yellow, or a yellowish white.

After the third fusion the regulus is absolutely pure, and exempt from copper, arsenic and iron. It has a silvery whiteness and is very splendent. Slowly cooled, its fracture is fine granular,—rapidly cooled, it is in broad laminæ. Before the blowpipe on charcoal it melts into a globule with a pure, splendent surface. If a current of air from a blowpipe be directed on a red hot globule, the metal is volatilised, in a state of oxide, in dense white fumes, *completely inodorous*, and burns away entirely without recourse to the flame. If a globule of the melted metal be slowly cooled on charcoal, after being heated to redness, it becomes surrounded with a circle of pearly oxide in fine needles, *perfectly white, or rather of a pearly white.*

When deflagrated with nitre, it yields diaphoretic antimony of a brilliant white; if boiled with nitric acid, it furnishes no trace of iron or copper, a proof that the metal is sufficiently pure.

The impure regulus is immediately recognised by the manner in which it is affected by the blowpipe. It melts with rather greater difficulty on charcoal than the pure metal; the melted globule is soon covered by a dross of sulphuret of iron, or other metal; the surface becomes tarnished and the metal does not continue to burn when the flame is withdrawn. The coating of oxide on the charcoal is not white, but more or less yellow: the fumes have an odour of arsenic.

The addition of sulphuret of antimony is designed to convert all the arsenic, and part of the iron and copper, into sulphurets. By fusion with carbonate of soda, these sulphurets combine with the soda without leaving any metallic residue, and there is no sulphuret of antimony dissolved or transformed into liver of antimony, as long as there remains any non-oxidised arsenic in the regulus of antimony.

The mixture must not be left in fusion more than an hour, otherwise the carbonate of soda attacks and dissolves the matter of the crucible, and the risk is increased of losing some of the metal by the separation of the upper part of the crucible.

The dread which type-founders have of the fumes of their melted metal is well known. When breathed, the effect of the arsenic on the system is soon perceived, and the presence of this metal in the antimony of commerce is the principal cause of these accidents.

If the price of metal exempt from arsenic could be rendered unobjectionable, it is probable that it would be ever preferred,—for, in addition to considerations of health, compositions of the purified metal melt more kindly and agreeably, and the types are equally as hard, and much less liable to injury by the action of the air. G.

Jour. de Pharmacie.

Preparation of Iodine. By M. Bussy.

The process of extracting iodine generally followed, and which consists in decomposing the mother-waters of kelp by means of concentrated sulphuric acid, is well known to be liable to variable results, on account of a portion of the iodine coming over in the state of hydriodic acid and chloride of iodine, so that in either case there is always a considerable loss.

To avoid this inconvenience, M. Soubeiran proposed to precipitate the iodine from the mother-waters by means of sulphate of copper, and afterwards to decompose the iodide of copper by peroxide of manganese at a high temperature.* But this process requires minute attention and much precaution to separate the whole of the iodine existing in the mother-waters, and we do not believe that it has ever been employed in any manufactory.

These circumstances induce me to publish a far more simple method, which has been lately employed by some manufacturers of iodine; it was discovered (if we are rightly informed) by M. Barruel, director of the chemical works at the Faculty of Medicine; it consists in precipitating the iodine of the mother-waters of kelp by a current of chlorine.

In this process the mother-waters are first to be evaporated to dryness, and to the residue there is to be added a tenth of its weight of powdered peroxide of manganese; the two substances are to be perfectly mixed, and subjected to a dull red heat in an iron vessel, frequently stirring them; the object of this calcination is to convert the sulphurets and hyposulphates, of which the mother-waters contain a large quantity, into sulphates. It is extremely easy to determine when these compounds are converted into sulphates by taking a small quantity of the calcined matter and pouring upon it an excess of sulphuric acid. It ought not to yield, when the conversion is perfect, either sulphuretted hydrogen or a deposit of sulphur. If violet vapours are disengaged during the calcination, the heat must be lowered to avoid loss. When the sulphurets are decomposed, the residue is to be dissolved in a sufficient quantity of water to give a solution of sp. gr. 1.333; through this solution there is then to be passed a current of chlorine gas, taking care to stir it constantly with a glass rod: the liquor becomes of a deep brown colour, afterwards turbid, and deposits iodine in the form of a black powder; it is to be collected and distilled in a glass retort, in order to procure it in crystals. The only difficulty in this process is to determine the point at which the action of the chlorine should cease, that no excess may be used which would react upon the iodine precipitated. This excess of chlorine is especially to be apprehended, when it is also intended to separate from the same mother-waters the bromine which they contain.

In order to avoid adding excess of chlorine, the liquor which is supposed to be near saturation, may be suffered to settle for a short time, and the current of chlorine being interrupted, it is to be directed on the surface of the liquid. If it contain any iodine in solution, a pellicle of iodine will be observed to form on the surface; this effect is not produced when all the iodine is precipitated; in the latter case the liquor becomes quickly clear and retains only a reddish tint.

Lon. & Ed. Philos. Mag.

Preparation of Bromine. By M. Bussy.

The separation of bromine, as usually performed, also involves great difficulties, which may be avoided by the following process.

* *Journ. de Pharmacie*, tom. ii. p. 411.

This process greatly resembles the preceding one; it is founded, like, it upon the great affinity of chlorine and the property which it possesses of displacing bromine from its combinations. It also includes the employment of the mother-waters of iodine, which without it are useless. Take the mother-waters of kelp, after having separated the iodine by chlorine as above described. These mother-waters contain a metallic bromide, when care has been taken to avoid the use of more chlorine than sufficient to precipitate all the iodine. Add to 1250 parts of these mother-waters, thirty-two parts of powdered peroxide of manganese, and twenty-four parts of sulphuric acid of sp. gr. 1.848: the whole is to be put into a tubulated glass retort, to which a tubulated receiver is to be adapted; and to this a tube which is immersed in a test tube. The retort and the receiver, as well as the receiver and the tube, ought to fit sufficiently well without the use of lute or corks, as they would be inevitably destroyed by the action of the chlorine. All being thus arranged, the retort is to be heated, so as to boil the liquid; the bromine condenses in the receiver in the form of red oily striæ, with a small quantity of water; the operation is to be discontinued when red vapours cease to be produced. On heating the receiver gently, without dismounting the apparatus, the bromine will pass into the test-tube, where it will condense on cooling.

The mother-waters which have been made use of may be rejected, on ascertaining, by the addition of a fresh quantity of sulphuric acid and peroxide of manganese that they contain no more bromine.—*Journal de Pharmacie*, January 1837. Ibid.

On the Extraction of Copper from Poor Ores, as practised successfully in Germany.

This process was first suggested by M. Karsten's observations in his "System of Metallurgy," in which he says:—"Very poor ores of oxydized copper, which could not be brought with advantage to the furnace, may be *roasted*, if mixed with pyrites of iron, or submitted in any way to the action of sulphuric acid gas, and will then form sulphates under the influence of the humidity of the atmosphere, or with water. Another method of extracting from similar ores, or even from the refuse, or scoria, is employed at Rheinbreitenback; and consists in mixing the old scoria of copper with old ores, and placing them in frames or boxes with numerous small holes in the bottom. A mixture of nitrous and sulphurous gas is injected into the centre of these boxes, with steam at certain intervals. As soon as the sulphuric acid is formed, it acts upon the metallic oxides by producing vitriol and copperas; besides which, the sulphuric acid acting only on the oxide of copper, a *sulphite* of this metal arises, which is speedily transformed into a sulphate, and the metallic salts being separated in the usual manner, they are again concentrated, and the copper is precipitated with heat by means of old iron; the copper thus obtained is afterwards mixed with flour of sulphur, then heated in a reverberating furnace; the result is the formation of sulphate of copper, which being dissolved with water and crystallized, is fit for the purposes of commerce. The liquid from which this copper is procured, is used for obtaining sulphate of iron."—*Annales des Mines.* Mining Jour.

Whiting's Patent Sashes.*

Your correspondent A. W. S. very justly complains of the annoyance oc-

* ——— Whiting, Esq., architect and county surveyor, Ipswich.

casioned by the rattling of the sashes, which prevails more or less in most houses with sash windows; in the majority of these cases, the cause I take to be (with all due deference to the opinion of Mr. Medcalf, Vol. III. p. 238,) the inferiority of the materials of which they and the frames are composed. I have seen a specimen of "*Whiting's patent sashes and frame*" at the house of his agent here, which appear to me to be well adapted for avoiding the above-named annoyance, and require only to be better known to insure their more extensive use. The frames are of solid deal, in which are square sinkings for the reception of the leaden weights; over these are fixed beaded linings, which also conceal the lines, and form small grooves to receive the metal tongues, fixed in the styles of the sashes: a metal tongue is fixed in the head, and another in the sill of the frame, catching the top and bottom rails when the sashes are shut; the meetings are beveled, and fastened with a common brass sash-fastener. The whole forming a very compact air and water tight window. Arch. Mag.

A Plan by which the Lines, &c., of hung Window-shutters may be concealed.

A friend of mine, a few years back, executed a plan which perfectly succeeded in producing the above effect. The shutters are made in two heights, hung as mentioned by Eboracum; and they slide down into a case, the side of which is formed by the window front. This case is closed at top by a flap, hinged on the outer edge, which conceals the shutters when they are down, and supports them when they are up. There are similar flaps to cover the pulley-styles and lines; but they are hinged on the inner edge, and they unite with the architrave, and effectually hide the lines, and keep the first-mentioned flap in its place when the shutters are down. These flaps are thrown back, when the shutters are up, between them and the sash bead of the window; sufficient space to allow of this, being left between it and the inner edge of the horizontal top. Ibid.

Indelible Inks.

No. 1.—For writing with quill-pens.

Indian ink, dissolved in water acidulated with hydrochloric* acid of commerce, and marking $1\frac{1}{2}°$ on Beaumé's areometer.†

No. 2.—For writing with metallic pens.

Indian ink, dissolved in water rendered alkaline by caustic soda, and marking 1° on Beaumé's areometer.

No. 3.—For printing the indelible designs and patterns.

Common printing-ink, whitened with a sufficient quantity of sulphate of barytes, either artificial or natural, which has been ground in water for a considerable time. Mag. Pop. Sci.

* Muriatic acid, about sp. gr. 1.28, so called by the French chemists.

† As Beaumé's areometer is but little used in this country, it may be useful to state that if water be taken at 1000 sp. gr.

1°	on this instrument is . .	1007 sp. gr.
$1\frac{1}{2}°$	ditto	1010 "
2°	ditto . . .	1014 "

For the acidulated solution.—To a pint of water add about three teaspoonfuls of muriatic acid.

For the alkalized solution.—To the same quantity of water add 100 grains of pure

Iron from Anthracite.

In Mr. Byer's lecture, on Galvanism, when under the head of Electro-magnetism, he exhibited a bar of cast-iron, eighteen inches long, and about half an inch square, which was made at the Yniscedwyn Iron Works, by the hot blast with anthracite. This bar, previous to the lecture, Mr. Byers placed within a helix of coated copper wire, about 100 feet long, and through which he passed two shocks from a small battery; the bar was subject to the action of the battery about two or three seconds, and when withdrawn from the helix, it was found to be permanently magnetised, exactly as if it had been a bar of cast-steel. Mr. B. stated he was not aware that *cast-iron* was affected in this manner, but it was known that common wrought-iron would not retain a charge of electricity so as to become permanently magnetic. Mr. B. therefore, concludes *this* iron (as now manufactured at Yniscedwyn) is similar to cast-steel, as far as its magnetic properties are considered.—*Merthyr Guardian.* Mining Journal.

Stone Coal Iron.

What we call stone coal, (the learned name anthracite) from the Greek word anthrax, which means charcoal. We understand that a short account is about to be published respecting the newly invented mode of manufacturing iron with that coal; in the mean time we can inform our readers, on good authority, that from repeated experiments made in the presence of numerous persons, it is found that a rail of a certain size, made in the old process, breaks under the weight of twelve tons, and a rail of precisely the same shape and dimensions, made of stone coal iron, broke under fourteen tons and a half; this is of importance in railroads. The stone coal iron is extremely pure, devoid of phosphorus, sulphur, and all other obnoxious substances, and bears a close resemblance, in quality, to foreign iron made of charcoal. One ton of iron is produced from a ton and a half of stone coal in the furnace, and eight hundred weight of small refuse coal for the heating apparatus, whereas two and a half tons of bituminous coal, are required to make a ton of pig-iron, and seven tons to make a ton of bar-iron. Of course the quantity varies according to the quality of the coal; but, the above will be found a fair average.—*Merthyr Guardian.* Ibid.

Present amount of Steam-power in Birmingham.

The number of steam-engines actually working at the present time in Birmingham, is *one hundred and sixty-nine*, amounting in the aggregate to

caustic soda, of the kind prepared for chemical purposes.

About 40 grains of Indian ink may be dissolved in a pint of the solution. This will make an ink sufficiently black for ordinary writing, but it is evident it may be very easily made darker, or lighter, at pleasure.

The Indian ink should be of the best quality. Care should be taken that it be uniformly good throughout the stick.

An inferior kind, hard, gritty, and possessed of little colouring power, is sometimes imposed upon the purchaser, by the deceptive practice of attaching a little bit of good ink at each end of the stick. A good mode of examination is to break the stick in two: this defeats the above trick, and exposes the fracture; that of good ink is brilliant, angular, and clean; the bad, dull and earthy.

two thousand seven hundred horses-power. They are distributed as follows:

	Horses-power.
In grinding flour	275
— working metals	1770
— pumping water	279
— glass-grinding	87
— working wood	97
— making and glazing paper	44
— grinding clay	37
— grinding colours and chemicals	61
— sundries	50
	2700

[*Birmingham Philosophical Institution. Report of Committee*, 1836.]

Mag. Pop. Science.

Progress of Physical Science.

On the Physical Causes of the principal Phenomena of Heat.

By John Barton, *Esq.*

It has always appeared to me that the corpuscular hypothesis is capable, with some modification, of affording a more complete and satisfactory explanation of the phonomena of heat and light than the undulatory hypothesis. On the present occasion I propose, without entering into any controversial discussion, to show in what manner the principal phenomena of heat may be deduced from the action of two forces. An attractive force between the particles of heat and those of solid matter, and a repulsive force between the particles of heat themselves.

I assume that the particles of heat are very small in comparison of the particles of solid matter, and that these last are very small in comparison of the intervals by which they are separated from each other. The phenomena also require that the repulsive force should decrease more rapidly than the attractive force. These premises being admitted, the following consequences may, if I do not mistake, be deduced from them. For brevity's sake I omit the demonstrations, presuming that they will be readily supplied by those versed in mathematics.

1. If a particle of heat approach a particle of solid matter, it will either fall to the surface and remain there, or will describe a curvilinear orbit thereabout. According to the direction and velocity of its approach, this orbit will either be confined within a certain limit, like an ellipse, or will go off to an infinite distance, like an hyperbola. To avoid circumlocution, I will take leave to call these two classes of curves respectively, *ellipsoidal* and *hyperboloidal* curves. A particle of heat reposing on the surface of a particle of solid matter, or revolving about it in an ellipsoidal curve, will have no tendency to fly off, or to pass into a contiguous body, unless disturbed by some interior force. It is therefore *latent*.

It must not be supposed that latent heat exists only in fluid or gaseous bodies. The phænomena of softness and malleability must be considered as resulting from the presence of a portion of heat in this state. Nay, it

is probable that even the hardest and most brittle substances are not entirely deprived of it.

2. When a solid body is exposed to friction, or to percussion, a certain number of the particles of heat which had revolved tranquilly in ellipsoidal curves are forced out of their orbits, and fly off beyond the reach of the attractive force of the particle to which they belonged; just as our earth might be driven out of the limits of the solar system by the stroke of a comet; supposing the latter of sufficient density and magnitude. And thus we have an explanation of the heat produced by friction, or percussion; which has been supposed to form one of the strongest objections to the corpuscular hypothesis.

It confirms this theory, that a piece of iron which has been heated by hammering becomes *brittle*; indicating that a portion of its latent heat has been lost, while sensible heat has been disengaged.

3. If we suppose a particle of matter to be of an oblong form, or to have one of its axes greater than the others, the particles of heat will collect chiefly about the middle of its length. And thus we have an explanation of a remarkable fact discovered by M. Mitscherlich, that crystalized bodies, when heated, do not expand equally in all directions.*

4. When the coefficients of the attractive and repulsive forces bear a certain relation to each other, and to the original velocity of approach, the orbit described will resemble a *conchoid*, its two branches being ultimately in one and the same line. In this case the body is *diathermanous* the rays of heat passing through it apparently in a straight course. The course of a ray through a solid body can never in fact be straight, unless its velocity be infinite; but in proportion as the velocity approaches this limit, the ray will experience less disturbance in passing through bodies imperfectly diathermanous. And accordingly it is found, that rays from incandescent bodies pass through some media which do not admit the passage of rays issuing from bodies of lower temperature, the velocity of projection evidently depending on the repulsive force; and this, in its turn on the temperature.

5. It must not however be supposed that opaque or *adiathermanous* bodies are impervious to the rays of heat. They differ from transparent or diathermanous bodies only in this, that the entering ray, instead of passing through immediately, and escaping from the opposite side, is entangled in a circuitous and irregular course among the particles, until by chance it reaches the surface, when it escapes, unless the angle of its direction with the tangent be so small that it is drawn back again by the attractive force. A particle of heat is therefore detained much longer in passing through an opaque than through a transparent body, and contributes much more to raise its temperature, and hence it also appears why the passage of heat through opaque bodies is very much slower than through transparent media.

6. The particles of solid bodies are drawn towards each other by two forces, their mutual attraction, and their attraction for the atmospheres of heat about the others. They are also kept apart by two forces, the mutual repulsion of those atmospheres of heat, and the repulsion of the particles of free or sensible heat, which happen at the time to be passing between them. As long as the number of these last remains unchanged, the balance of the opposing forces is a balance of stable equilibrium, in as much as the attractive force diminishes with the distance in a less ratio than the repulsive force.

*See Phil. Mag., First Series, vol. lxiv. p. 162; and Lond. and Edinb. Phil. Mag. vol. i. p. 413.—Edit.

If the number of particles of sensible heat is increased, the body dilates till the equilibrium is restored; but if this dilatation proceed to a certain point, the orbits of a certain number of the revolving particle of heat will be changed from the *hyperboloidal* to the *ellipsoidal* form; that is to say, a certain quantity of sensible heat will be converted into latent heat. At the same time another portion of sensible heat will be acquired from the surrounding bodies and the united repulsive force of these two portions of heat will overbalance the forces of attraction. Thus we have an explanation of the phenomena of *vaporization.*

An account of the causes of this last class of phenomena somewhat resembling the above has been given by Laplace in the twelfth book of the *Mecanique Celeste.* Laplace however gives no explanation of the difference between latent and sensible heat: he attributes the phenomena of expansion exclusively to sensible heat, but does not explain why heat when it becomes latent should lose its repulsive force: he supposes the particles of heat to form atmospheres about the particles of solid matter, which are at rest unless disturbed; and in order to explain why sensible heat tends continually to fly off, he supposes the particles of matter to be in a state of continual agitation.

7. In the same chapter Laplace has given an explanation of the phenomena of liquefaction, which appears to me to require modification. "Every molecule of solid matter," he says, "is subjected to the action of three forces: 1st, the attraction of the surrounding molecules; 2ndly, the attraction of the caloric of those molecules, *plus* their attraction for its caloric; 3dly, the repulsion of its caloric by the caloric of those molecules. The first two forces tend to bring the molecules nearer together, the third to separate them. The three states of *solidity*, *fluidity*, and *gaseous elasticity* depend on the respective efficacy of those forces. In the state of solidity, the first force is the most powerful; the influence of the figure of the molecules is very considerable, and they are united in the direction of their greatest attraction. The increase of caloric lessens this influence by dilating the body, and when this increase is such that the influence in question is very small, or none, the second force predominates, and the body takes the liquid form."

I am unable to comprehend why the attractive force between two solid particles should be suddenly reduced to nothing when they are separated to a certain distance one from the other. And this suggestion appears still less probable when it is considered that many crystalizable bodies cease to contract, and even undergo a degree of expansion, at the moment of congelation. The act of changing from fluid to solid depends then on some other cause than the approximation of the particles. Laplace has, I think, truly assigned the immediate physical cause of solidity, when he says that in solid bodies the particles are placed relatively to each other in the position of greatest attraction; but he has not given an adequate reason for their assuming this position. To supply this deficiency, it may be useful to revert to an explanation above given of an observation of Mitscherlich on the unequal expansion of crystalized bodies in different directions. It was suggested that the particles of heat do not accumulate equally round every part of a particle of solid matter if its axes are of unequal length. Now, this inequality of distribution is such, by the nature of the case, as to counteract the tendency of the particles to place themselves in the position of greatest attraction. In other words, the compound force exerted by the central solid nucleus, with its surrounding atmosphere of heat, will ap-

proach more and more nearly to that of a sphere as the heat increases; and by consequence the figure of the particles exercises a less and less influence on their mutual position. When that influence is completely neutralized, the body is a fluid.

8. As the mutual attraction of the particles is not however destroyed in fluids, these particles still arrange themselves in the position of greatest attraction. But this position is no longer influenced by their figure; it is now such, precisely, as would be assumed by them if spherical, that is to say, the position in which the distance of their centres is a minimum. Now this is the position in which the sum of the intensities between them is also a minimum; therefore the whole bulk of the fluid just before congelation is a minimum. If we suppose other forces to come into operation, those, for instance, resulting from the figure of the particles, the dimensions of the body will consequently be enlarged; and thus it appears why so many substances are found to expand in the act of congelation.

9. Further, as the position of greatest attraction amongst the particles of a solid is that in which their salient angles are presented towards each other, it follows that at the moment of solidification, when this arrangement is established, the orbits of a certain portion of the accompanying particles of heat will be changed from the *ellipsoidal* to the *hyperboloidal* form; that is to say, a certain quantity of latent heat is converted into sensible heat.

10. Those solid bodies which are composed of the largest, or rather the heaviest, particles, are at once the best conductors, and the worst radiators, of heat; for the velocity increases with the mass. Hence the metals are the best conductors of heat, and as far as the experiments hitherto made enable us to judge, their conducting power seems to follow the same order with the magnitude of their component atoms, the differences not being greater than may be reasonably supposed to arise from errors of observation. For a similar reason, a particle of heat attempting to escape from the surface of a metallic body is more strongly drawn back by the attractive force of the particles than at the surface of other bodies. The metals have therefore less radiating power than any other substance.

11. The particles of heat emerging from the surfaces of solid bodies at angles so small as to be drawn back again into their substance, form collectively an atmosphere enveloping those surfaces, and extending, there is reason to believe, to distances considerably greater than the interval which separates the solid particles from one another. It is to this atmosphere that we must attribute the repulsive power exerted at the surfaces of bodies, metallic bodies especially, by virtue of which mercury is depressed in a barometer tube, and a steel needle floats on the surface of water. By strong pressure this enveloping atmosphere may be expelled from between two metallic surfaces, and the attractive force of their particles then coming into play, they adhere with considerable force. The reflection of heat at the surfaces of bodies also appears to be due to the action of this enveloping atmosphere.

12. It may facilitate the apprehension of my meaning to observe, that a particle of matter, with its revolving particles of heat, is supposed to have a resemblance to the sun, with its accompanying bodies, in the solar system. The planets, revolving in elliptic orbits, represent the particles of latent heat; the comets, if indeed any of them revolve, as formerly supposed, in parabolic or hyperbolic orbits, represent the particles of sensible heat. Future observations will, perhaps, enable us to determine the law of the forces by which these minute movements are regulated, as accurately as we are

now acquainted with the law of gravitation. The preceding conclusions are, however, independent of the particular law of those forces.

January, 27, 1837.

P. S. I have assumed, in conformity with the views of Laplace and other mathematicians, that the particles of solid matter mutually attract each other. But the preceding conclusions hold good though no such force of attraction exist, or even if we suppose, with Æpinus, that the particles mutually *repel* each other, a supposition which is by no means incompatible, as Dr. Roget has observed,* with the phenomena of gravitation. There are certainly strong reasons in support of this last hypothesis.

Lon. & Ed. Philos. Mag

Simultaneous Meteorology.

The value of the plan of observation suggested in the following paper, coming as it probably does, from one of the most distinguished philosophers of the present day, will be acknowledged, we trust, by every reader; and we cannot doubt that in various parts of the United States, observers will be found, who, as far as their means extend, will carefully practice upon these suggestions. If no central point for the reception of the Registers made in this country, should occur, preferable to the Franklin Institute, they will be cordially received by the Actuary, and properly disposed of by a committee of the Institute. Independently of the benefit which such simultaneous American observations may confer upon Science by being forwarded to the Cape of Good Hope, they will have a high value in reference to the progress of meteorological knowledge among ourselves, and come into admirable unison with the encouragement now afforded by some of our state governments, (and which we hope to see adopted in time by all of them) for the procuring of regular and accurate Registers of observation throughout their limits. Hoping to receive from more places than one in the United States a series of observations, made at the time of Equinoxes and Solstices as recommended in this paper, and upon the plan of those given in page 192, we shall continue to insert, from the foreign Journals, one table of the hourly observations made in England at those times, for the purpose of comparison. G.

In some "Instructions for making and registering Meteorological Observations," circulated by the South African Literary and Philosophical Institution, established at the Cape of Good Hope, and drawn up, it is believed, by Sir John Herschel, there is, among others, the following recommendation :—

"With a view to the better determining the laws of the diurnal changes taking place in the atmosphere, and to the obtaining a knowledge of the correspondence of its movements and affections over great regions of the earth's surface, or even over the whole globe, four days in each year should henceforward be especially set apart by meteorologists in every part of the world, and devoted to most scrupulous and accurate registry of the state of the barometer and thermometer; the direction and force of the wind; the quantity, character, distribution of clouds; and every other particular of weather, throughout the whole twenty-four hours of those days, and the adjoining six hours of the days preceding and following.† The days

*See Scientific Memoirs, Part III. p. 469.—Edit.

† This is necessary by reason of the want of coincidence of *the day* in different parts of the globe, arising from difference of longitude. In order to obtain a complete correspondence of observation for twenty-four successive hours over the whole globe,

recommended for these observations are, the 21st of March, the 21st of June, the 21st of September, and the 21st of December, being those, or immediately adjoining to those, of the equinoxes and solstices, in which the solar influence is either stationary, or in a state of most rapid variation. *But should any one of those 21st days fall on Sunday, then it will be understood that the observations are to be deferred till the next day, the 22nd.* The observation at each station should commence at 6 o'clock A. M. of the appointed days, and terminate at 6 o'clock P. M. of the days following according to the usual reckoning of time at the place. During this interval, the barometer and thermometer should be read off, and registered hourly, and the precise hour *and minute* of each reading should be especially noted.

"For obvious reasons, however, the commencement of every hour should if practicable, be chosen, and every such series of observations should be accompanied by a notice of the means used to obtain the time, and, when practicable, by some observation of an astronomical nature, by which the time can be independently ascertained within a minute or two.* As there is scarcely any class of observations by which meteorology can be more extensively and essentially promoted, it is hoped that not only at every station of importance in this colony, but over the whole world, and on board ships in every part of the ocean, individuals will be found to co-operate in this inquiry. Every communication of such observations, addressed by channels as secure and as little expensive as possible, to the Secretary of this Institution, will be considered as highly valuable."

In cheerful compliance with the above valuable recommendation, the observer to whom is confided the formation of our Meteorological Table, undertook the proposed series of observations during the period which has lately passed. The results will be found in the Table which follows, and which we transmit to the Secretary of the Institution at the Cape, as our contribution to the collection which is there proposed to be amassed and concentrated from the whole surface of the globe.

Observations of the same nature, and at the same times, have been taken at the Royal Observatory, Greenwich; at the apartments of the Royal Society, Somerset House; at the Observatory of Cambridge, &c.

As one of the most important ends of meteorological observations is their comparison with each other, and as, for this purpose, it is very desirable that observers should understand the use and adjustment of the instruments in their possession, we have been induced to select from these instructions such of them as are of general application.

I.—*General Recommendations and Precautions.*

"1. The continuity of observations ought to be interrupted as little as possible by changes in the adjustments of instruments—in their places—exposure—mode of fixing—or of reading off and registering them. Whenever any alteration in these, or any other particulars, takes place, especially such as are likely to effect the zero points, or otherwise to influence the mean results, it should be noticed in the register.

it must be taken into account that opposite longitudes differ twelve hours in their reckoning of time. By the arrangement in the text, the whole of the *astronomical day*, (from noon to noon) is embraced in each series, and no observer is required to watch two nights in succession.

* For example, the first appea ances and last disappearances of the sun's upper and lower border, above and below the sea-horizon, if at sea or on the coast,—or on land the exact length of the shadow of a vertical object of a determinate length on an horizontal level, at a precise moment of time (not too near noon,) &c.

TABLE of HOURLY METEOROLOGICAL OBSERVATIONS, made during the 21st and 22d of March, 1837, at **Blackheath road,** near Greenwich, about four and a-half miles to the south-east of London, by **J. H. Belville,** in conformity with the Instruction circulated by the South African Literary and Philosophical Institution.

Day.	Hour.	Barometer.	Therm. Attach'd	Therm. In open air.	Wind. Direction.	Wind. Strength	Clouds, &c.
		English Inches.	F. °	Fah. °			
Tuesday, March 21, 1837.	VI. A. M.	29.971	42	24.2	N. by W.	1	Snow on the ground. Intense frost. A few clouds to N. A Radiator on the ground had fallen to 19°
	VII.	29.967	42	26.8	N.	1	Clouds coming up. Sunshine through thin clouds.
	VIII.	29.962	39	28.2	N.	1	Clouds collecting, and overspreading the sky. *Cirro-stratus.*
	IX.	29.943	36	29.6	N.	1	Clouds increasing. Snow coming on from the northward.
	X.	29.928	35	30.0	N.	1	Snowing. *Cirro-stratus.*
	XI.	29.903	35	29.0	N. N. W.	2	Small snow. [*muli.*
	XII.	29.892	36	31.2	N.	1	Clouds broken. *Cirro-stratus* into *cu-*
	I. P. M.	29.853	36	31.1	N.	1	Clouds broken. Snow still falling.
	II.	29.848	37	31.5	N.	1	A shower of granular snow has just fallen.
	III.	29.843	37	33.0	N.	1	Large dense *cumuli.* Clear sky be.
	IV.	29.836	39	33.4	N. E.	1	*Nimbus* advances from N. [tween-
	V.	29.839	38	31.5	E.	0	Shower of snow. Wind changes to E.
	VI.	29.841	39	29.8	E. S. E.	1	Snow, beautifully crystallized of a *hexagonal* form, falls, and remains on the ground.
	VII.	29.839	38	28.9	S. E.	1	Clearer. Moon visible.
	VIII.	29 838	42	28.7	S. E.	1	Clouds again collect from the N.
	IX.	29.830	39	28.6	S. S. E.	0	Getting quite overcast. Very cold.
	X.	29.830	42	28.4	S.	0	Generally cloudy, and calm.
	XI.	29.824	43	28.8	S.	0	The same.
	XII.	29.818	43	28.7	S. W.	0	The same.
Wednesday, March 22.	I. A. M.	29.848	48	28.7	S.	0	The *cirro-stratus* clouds prevail.
	II.	29.797	43	28.7	S.	1	Sky quite covered, as before.
	III.	29.798	43	28.6	S.	0	The same, } Night extremely cold.
	IV.	29.796	43	28.6	S.	0	The same. } ground hard with frost.
	V.	29.796	42	28.0	S. W.	0	The same. } Ice nearly an inch thick
	VI.	29.798	42	27.9	S.	0	Clouds broken to the east. Sun visible at his rising. [quickly from N.
	VII.	29 81 4	42	28.5	S. E.	1	Clouds getting thinner, and moving
	VIII.	29.836	42	29.1	E. S. E.	1	Getting clear, particularly to the N. E.
	IX.	29.838	39	31.8	E.	1	Still clearer. Sun Shining bright.
	X.	29.839	40	34.0	E.	1	Fine. *Cumuli* at a distance.
	XI.	29.842	41	36 1	E.	2	The same.
	XII.	29.858	42	38.4	S. E.	1	*Cumuli* collect apace.
	I. P. M	29.868	43	37.7	S. E.	1	More overcast. [snow fall.
	II.	29.864	43	37.9	E. S. E.	1	*Cumuli* into *cum.-stratus.* Flakes of
	III.	29.866	43	36.8	S. E.	0	Thick. No blue sky visible.
	IV.	29.866	42	36.1	E.	0	The same. Upper atmosphere hazy.
	V.	29.870	42	33.0	E.	2	Clouds thinner in zenith. Air feels cuttingly cold.
	VI.	29.904	42	31.5	E.	1	Getting quite cloudy and thick.

Notes.—The time was taken from a good clock, keeping nearly mean time, its rate being scarcely perceptible, and its error obtained by observation of the Ball-Drop of the Royal Observatory, Greenwich.

The Barometer has an elevation of about forty feet above the River Thames.—Ma-

"1. So far as possible, registers should be complete—but if, by unavoidable circumstances of absence, or from other causes, blanks occur, no attempt to fill them up by general recollection, or by the apparent course of the numbers before and after, should ever be made.

"3. The observations should, if possible, all be made by one person—but as this may often be impracticable, the principal observer should take care to instruct one or more of his family how to do it, and should satisfy himself by many trials that they observe alike.

"4. The entries in the register should be made at the time of observation, and the numbers entered should be those actually read off on the respective scales of each instrument, on no account applying to them previous to entry *any sort of correction, as, for instance, for zero, for temperature, capillarity, &c.* All these and the like corrections, being matter of calculation and reasoning from other observations, are to be reserved till the final discussion of the series, and for separate determination and statement.*

"5. If copies be taken of the registers, they should be carefully compared with the originals by two persons, one reading aloud from the original, and the other attending to the copy, and then exchanging parts, a process always advisable wherever great masses of figures are required to be correctly copied.

"6. The register of every instrument should be kept in parts of its own scale, as read off, no reduction of foreign measures, or degrees, to British being made—but it should of course be stated *what* scale is used in each instrument.

II.—*Of the Times of Observation and Registry.*

"Meteorological observations should be made and registered daily, at stated and regular hours. In fixing on these, some sacrifice of system must of necessity be made to the convenience and habits of the observer. The best hours in a scientific point of view, would be those of Sun-rise, Noon Sun-set, and Midnight. But these are not the hours adapted to general habits; and since the midnight observation is likely to be pretty generally neglected elsewhere than in an Astronomical Observatory, the following hours, for a division of the day into three parts, are proposed for what may be deemed the Morning, Afternoon, and Evening observations, viz:

Morning . . .	8 A. M.
Afternoon . . .	2 P. M.
Evening . . .	8 P. M.

"If, however, the habits or engagements of any one should not allow him to conform to these hours, rather than not observe he may select his own, specifying only what they are at the head of every page of his register, and adhering steadily to them in practice, only observing to make the extreme observations of each day equidistant from the middle one.

"At the same time it will be borne in mind, that, in what concerns great meteorological questions on which the most interesting features of the subject depend, the night is quite as important as the day, and has

kers, Watkins and Hill, London.——The Thermometer has a northern aspect.

The strength of the wind is indicated by the figures in the column, thus—0 marks that there was *no wind perceptible*; 1, *a very light breeze*; 2, *a strong breeze.*

*We regard this as of the highest importance.

hitherto been far too much neglected. To any one, therefore, who may feel disposed to enter more zealously into the subject, and will not consider some personal inconvenience ill undergone for the sake of affording data of a peculiarly valuable description, this Committee would most earnestly recommend the adoption, in preference to all others, of the quaternary division of the 24 hours.

III.—*Of Meteorological Instruments,—and, first, of the Barometer and its attached Thermometer.*

"The Barometer is the most important of all meteorological instruments. Its office is to measure the actual pressure of the atmosphere on a given horizontal surface at the time and place of observation. Its fluctuations are observed to have considerable relation to changes in the weather, and especially of the wind. Hence its use as a weatherglass.

"A barometer should be examined, before setting it up, for air-bubbles in the tube, and for the existence of air above the mercury in the upper part of the tube. This is done by gently inclining the instrument either way from the horizontal position a little up and down; when air-bubbles, if large, will be seen to run to and fro, and must be evacuated by inverting the instrument, and by gentle blows on it with the hand, driving them up into the cistern. If this cannot be done, the instrument is useless. If air exists to an objectionable amount *above* the quicksilver, it will not tap *sharp* against the upper end of the tube, when the barometer is quickly inclined from a vertical position, so as to make the mercury rise above its level, nearly to the top, and then gently *jerked* lengthways and backwards. If the blow is buffy and dead, or is not heard at all, the amount of air must be considerable, and may be expelled by inversion.

"In fixing the barometer, choose a good light near a window, but not exposed to sunshine, in a retired apartment little liable to sudden changes of temperature or to draughts of wind. Adjust the table to a vertical position by a plumb-line, and fix it so as never to shift from that position. Before reading off, give a few taps, on the instrument, enough to make the upper end of the column of silver *shake* visibly, as the mercury is apt to adhere to the glass, and give erroneous readings. In reading, bring the index always opposite to one part. The correct part to choose is the summit of the convexity of the mercury, to which the index should be made a tangent; but if this be difficult to hit, either from the construction of the index or the want of a proper fall of light, the line of junction of the mercury and glass may be taken. In that case, the tapping should never be omitted. Which ever mode of reading is once adopted, should be stated, and always adhered to. A piece of white paper placed behind the upper part of the tube will generally enable any one to read off by the convexity of the quicksilver. In placing the index, notice whether it appears to shift a little up and down as the eye is raised or depressed: this is called parallax, and is a source of uncertainty, to be avoided by placing the eye in reading always on the *exact level* of the top of the mercurial column.

"Barometric observations require corrections of three kinds; and to render them available and comparable with others, it is necessary that their amount should be ascertained, and distinctly stated. The first is called the Zero Correction. It includes several subordinate corrections arising from different sources, such as that originating in a faulty placing of the scale of inches, that due to the capillary depression of the mercury in the glass-tube, and the constant part (which at a fixed station is nearly the

whole) of the depression arising from the presence of air or vapour in the upper part of the tube.

"To determine the zero correction, the barometer must be compared with a standard instrument, such as that at the Royal Observatory for instance, or some other which has been compared with it, or with some standard of equal authority. Such comparison ought never to be omitted before forwarding the barometer to its place of destination, nor should any opportunity be neglected of comparing it, when fixed in its place, with a good portable barometer. In making such comparisons, all that is necessary is to record the readings of both the instruments, after, at least, an hour's quiet exposure, side by side, that they may have the same temperature. If compared by two observers, each should read off his own barometer in his usual manner, and each should take a mean of several readings, then each should verify the other's results. By this means the zero of one standard may be transported over all the world, and that of all others compared with it, ascertained.

"The amount of the zero correction is often very large, as two or three tenths of an inch, but its influence on the mean results of recorded observations, falls wholly on the determination of the heights of the station of observation above the mean level of the sea, and effects little, if at all, any conclusions of a meteorological nature which may be deduced from them. Hence, if proper care be taken to preserve a barometer, once set up, immovable, a long and regular series of observation with it has a value independent of any knowledge of this element, and it is fortunate that this is the case, as the zero correction is one extremely difficult to determine exactly *a priori*.

"In transporting a *compared* barometer to its place of destination, great care is necessary. It should always be carried *upright*, or considerably inclined, and *inverted*, and over all rough roads should be carried in the hand, to break the shocks to which it would otherwise be exposed. Strapped obliquely across the shoulder of a horseman, however, it travels securely and well, and with common care in this mode of transport, its zero runs no risk of change.

"The next correction, and the most important of all, is that due to the temperature of the mercury in the barometer-tube at the time of observation. To obtain this, every barometer requires to have attached to, or fixed very near it, a thermometer, called the attached thermometer, which must be read and registered at each observation of the barometer. It is preferable in practice to read off this thermometer *first* to avoid the error arising from breathing on, or standing long near it, while reading the barometer itself. The zero of this thermometer should be ascertained by comparison with a standard at the temperature of about 60° Fahr.

"The third correction applicable to barometric observations arises from change of level of the mercurial surface in the cistern, owing to the transfer of a portion of its contents to, or from, the tube. In barometers with small cisterns, and where the lower level cannot be adjusted at each observation, its amount may be large, and its effect being always to make the apparent fluctuation less than the real, *in a fixed proportion*, it ought, if possible, to be ascertained. The data necessary to be known are—first, the internal and external diameters of the tube—secondly, that of the cistern containing the mercury, at the surface, where the tube plunges into it. These particulars, as they must be known to the maker, ought be inquired of him, and indeed ought to be engraved conspicuously on some part of the instrument.

"Although all these corrections are necessary for the strict *reduction* of registered observations, they ought not to be applied to individual observations previous to registry. It is sufficient to know them. Their effect is in all cases easily and safely applicable to mean results, and to the conclusions therefrom deduced, and a world of troublesome, and often mistaken, calculations may be saved by so applying them.

"*Of the External Thermometer.*—The external thermometer should have a scale on which whole degrees are read off, and divisions large enough to admit of estimating tenths, or at least quarters of degrees, by the eye. It should be compared with a standard, and the difference stated, at one or more temperatures, (the wider asunder the better) within the range of the climate in which it is to be used. In fixing it, choose a perfectly shaded, but otherwise free, exposure, and one where no *reflected* sunbeams from water, buildings, rocks, or dry soil, can reach it, and easily accessible for reading. There fix it firmly and upright. In reading it, avoid touching, breathing on, or in any way warming it, by near approach of the person. The quicker the reading is done the better.

"Although read off at stated times, notice should be taken of all sudden and remarkable changes of temperature, as indicated by the external thermometer, whenever they occur.

"*Of the Maximum and Minimum, Self-registering Thermometer.*—This should be placed horizontally in some place out of doors, shaded from direct radiation and rain, and otherwise freely exposed to air, and so fastened as to allow of one end being detached from the fastening and lifted up, so as to let the indexes within the boxes slide down to the ends of the fluid columns; a more convenient mode, when the steel index is free enough to allow it, than the use of a magnet.

"Both the thermometers should be read off as early as possible every morning, and the indexes re-adjusted. But as double maxima frequently, and occasionally double minima, occur, in consequence of sudden changes of temperature, it is recommended occasionally to inspect both of them, with a view to ascertain whether the motion of either the mercury or spirit has been reversed in an unusual manner; and such double maxima or minima, when remarkable, should be recorded as 'supernumerary,' with their dates and leading features.*

"Self-registering Thermometers are extremely apt to get out of order, by the indexes becoming entangled in the column of fluid. In traveling, they should not for a moment be carried with the mercury bulb downwards; if this should happen, they are *sure* to arrive in a state unfit for use. To correct them is tedious, and always hazards fracture. With great care, however, it may be done, as follows:—

"1*st*. The Spirit Thermometer. By many jerks, force the index down to the junction of the bulb and tube; then, by cautiously heating and cooling alternately the bulb, the tube, or the air-vessel at the top, as the case may require, the disunited parts of the spirit may be *distilled* from place to place, till the whole is collected in one column in union with the spirit in the bulb.

"2*nd*. The Mercurial Thermometer. When the steel index gets im-

* "The spirit thermometer is apt to undergo a gradual change of zero by the transfer (by distillation) of part of its spirit to the upper end of the tube. It should, therefore, often be compared with the mercurial one, and the difference of readings *applied* as a zero. In this *only* case is the application of a zero *before* registering permissible, and indeed essential."

mersed in the mercury, it cannot be moved by a magnet, and lets the mercury pass by its side. First cool the bulb (by evaporation of ether, if necessary) till the mercury is either fairly drawn down below the index, or a separation takes place in the column, leaving the index with mercury above it. Endeavour then, by tapping, warming the tube, or by the magnet, to loosen the index ever so little, then apply heat to the bulb, and drive up the index, with its superabundant mercury, quite into the air-vessel. This requires many trials and much patience. When there, hold the instrument bulb downwards, and suspend the index by a magnet at the top, allowing any globule of mercury to drop into the origin of the tube below; then heat the tube cautiously over a very small clear flame of an oil lamp, till the mercury rises to the very top of the tube, and fairly unites with the globule there awaiting it. Let the bulb cool, and the mercury will sink in one united column; if not, heat it again. When this is accomplished, the index may be set loose, by withdrawing the magnet, and restored to its proper position in the tube.

"A self-registering thermometer may be advantageously left (properly secured) for a whole year, or parts of a year, on elevated summits or other remarkable points, to ascertain their maxima and minima of temperature during absence. In such cases, take care to defend them from discovery, or accident from animals, birds, snakes, &c. In taking it up for reading off, observe not to derange the indexes, and do not leave it without seeing that the indexes are in contact, and the temperature that of the air at the moment.

"*Of Thermometers buried in the Earth.*—Thermometers buried at different depths, for the purpose of examining the monthly changes of temperature of the soil, should have their balls and lower part of the scale well wrapped up in a woolen cloth or pounded charcoal, and should be placed in strong earthen vessels which may be entirely withdrawn from the ground so as to allow of inspecting and reading off the scale, without exposing the balls to any possibility of changing their temperatures while under examination. The vessels should be fitted with covers, to defend the scale from injury in burying and digging up.

"A pipe of earthenware (composed of separate pieces,) or one of wood, may be sunk ten or fifteen feet below the surface, into dry earth, and a thermometer, defended as above, lowered *by a chain*. The pipe being then obstructed at every two feet by some stuffing readily hooked up, the thermometer may be easily examined, and a register of its indications kept with very little trouble. In like manner, the temperature of wells may be registered."

[TO BE CONTINUED.]

A comparison of the late Imperial Standard Troy Pound Weight with a Platina copy of the same, and with other Standards of authority." Communicated by Professor SCHUMACHER, *in a Letter to* FRANCIS BAILEY, Esq. V. P. *and Treas. of the Royal Society.*

Professor Schumacher being desirous of procuring an accurate copy of the English Imperial Standard Troy pound weight, for the purpose of comparison with the Danish weights, applied to Capt. Kater, requesting him to cause such copy to be made; which was accordingly done. It was made of brass by Bate; but the result of the weighings not being satisfactory to

Professor Schumacher, he desired to have a second copy forwarded to him. As these two copies did not agree in their results, the first was returned to Capt. Kater with the request that he would repeat the weighings. The result confirmed Professor Schumacher's suspicions: and as it was not thought proper that, in an affair of so much importance as the comparison of the standard weights of two nations any source of discordance should exist, or even be suspected, (the preceding experiments having been made with a *copy* of the Imperial standardweight) the Danish Government sent over Capt. Nehus (of the Royal Danish Engineers) to this country for the express purpose of making comparisons with the *original* standard, in the possession of the Clerk of the House of Commons.

The weighings took place in the Apartments of this Society, and were partly made with Ramsden's balance, belonging to the Society. Besides the first brass weight above mentioned, there was another brass weight made by Robinson, a platina weight made by Cary, the brass pound weight belonging to the Royal Mint, and the platina pound weight belonging to this Society. These were all subjected to a most rigid and accurate series of weighings by Capt. Nehus, in which every precaution was taken to insure the most correct results. It would be impossible here to follow Capt. Nehus through all his details: but it may be sufficient now to state that upwards of 600 comparisons were made with the English Imperial standard, all of which are apparently very accordant; but, on account of a singular circumstance connected with the *original* standard, do not possess that degree of precision, nor afford that satisfaction which ought to attach to an affair of so much importance. For, it appears that not only the specific gravity of the original standard had never been ascertained, but that we are even ignorant of the kind of metal of which it was composed: some persons maintaining that it was of brass, others of copper, and others of bell-metal. And as the original was totally destroyed in the late fire which consumed the two Houses of Parliament, we cannot now supply this omission. It is well known that the specific gravity of brass may vary from 7·5 to 8·5; so that a difference of at least $\frac{1}{8}$ of a grain might arise from this circumstance alone; setting aside a number of other particulars that require minute attention, and which do not seem to have been attended to in former experiments of this kind. In fact, as Professor Schumacher remarks, though we have thus five different pounds in excellent preservation, and compared with the lost standard, with the greatest care and the best instruments, though the number of these comparisons exceeds 600, yet there still remains, an uncertainty as to its real weight; and this solely on account of specific gravity and expansion not being known. And, he adds, that it is to be hoped that no pound will in future be declared a legal standard unless these elements (the knowledge of which is indispensable even for a single comparison with a good balance) are previously determined with the greatest possible precision.

Besides the account of these numerous weighings, which are stated in detail, Professor Schumacher has given various formulae and tables which will be found of great use and application in any future experiments of a like kind that may be undertaken. Proceed. Royal Soc.—Lond. & Ed. Philos. Mag.

Fossil Infusoria.

One of the most remarkable discoveries of modern times is the fact ascertained by professor *C. G. Ehrenberg*, and communicated by him in sev-

eral successive memoirs to the Royal Berlin Academy of Sciences, that a number of siliceous rocks and minerals, and especially the hard substance used in polishing, called Tripoli, is composed almost entirely of the remains of infusory animals.

"We may regard (he states) as fully ascertained facts, that the following substances consist, either entirely or in part, of the shells of loricated infusoria, viz.

1. *Bergmehl,*
2. *Kieselguhr,* } Of the newest formation.
3. Polishing slate.
4. *Saugschiefer,* } Tertiary substances.
5. The semi-opals of the polishing slate,

We may consider the following minerals as *very probably* of the same description :—

6. The semi-opal occurring in Dolomite,
7. The precious opal occurring in porphyry, } Secondary and primary formations.
8. The flints occurring in chalk,
9. Yellow earth,
10. Bog iron-ore, } Of the newest formation.
11. Certain kinds of lithomarge.*

In a subsequent paper the author remarks:

Several years ago, the author discovered that the ochre-yellow slimy substance, which sometimes covers the soil in marshy rivulets and ditches, and seems often to be regarded as a deposition of oxide of iron, is a very delicate form of the Bacillarie, which, when heated, becomes red, like the oxide of iron, and is very ferruginous; but which neither loses its form by ignition, nor by the action of acids, and therefore must possess a siliceous covering, that approaches most nearly the genus *Gaillonella*. Hence, this form was, during the previous year, figured as the *Gaillonella ferruginea* in plate 10 of the work on Infusoria, which is about to appear. All the ochre surrounding bog iron-ore exhibits siliceous filiform members, as the residue after the removal of iron. The above appearances render if extremely probable that the Gaillonella ferruginea performs a very important part in the formation of bog iron-ore, whether it be by the direct addition of the iron contained in itself, or by attracting that belonging to neighbouring foreign substances.

The slaty Tripoli of commerce also consists almost entirely of infusory animals. The polishing slate of Bilin, in Bohemia, which forms whole strata, consists almost entirely of a minute infusory animal, which can be referred to the genus *Gaillonella* (*Gaillonella distans.*) The *Podosphenia nana*, n. sp., *Navicula scalprum?* and *Bacillaria vulgaris* (the two last are recent marine species) occur singly between the individuals of the prevailing species, and the first only can sometimes be compared to the Gaillonella for abundance. In the same polishing slate there occur impressions of plants and one species of fish, the *Leuciscus papyraceus* of Broon, according to Agassiz. In the adhesive slate of Menilmontant, some indis-

* The examination of a boulder found in the *Mark Brandenburg*, which had been regarded as a floatstone or *Schwimmstein* (Quarz-agathe nectique, H.,) has recently proved to me, that its principal mass consists of unattached siliceous spicula of sponges, and of the small globular bodies (infusoria, Pyxidiculis ?) which the flint pebbles of the district contain in great abundance. These bodies occur in the powder of the flinty envelope. The *Schwimmstein*, therefore, bears the same relation to the flint as the polishing slate to the semi-opal, and is to be referred to the chalk.

tinct traces were found of the *Gaillonella distans.* An individual of this animal which, almost without any uniting basis, constitutes the polishing slate of Belin, is $\frac{1}{288}$ of a line in size; many are smaller; there are consequently forty-one thousand millions of these animals in one cube inch of that substance. Ed. New Philos. Jour.

Progress of Civil Engineering.

On the Expansion of different kinds of Stone from an increase of Temperature, with a description of the Pyrometer used in making Experiments. By Alexander J. Adie, Civil Engineer.

(Read 20th April, 1835.)

For a long time I was anxious to know the rate of expansion of the common building stone of this neighbourhood, as it is not given in any of the tables of the expansions of substances, because I have sometimes thought that the vertical cracks frequently seen intersecting rubble walls might arise from the contraction caused by a diminution of temperature. During a long continued and severe frost which occurred in 1826, I thought the rents in a considerable stretch of wall, which I passed at all seasons, appeared more open than usual. This, however, was merely conjectural, and I paid no more attention to the subject until 1830, when, as I mentioned in a notice on the commencement of my experiments read at the meeting of the British Association in September last, an interdict of the Dean of Guild Court of Edinburgh, rendered the rate of expansion of stone a matter of more importance than merely a curious philosophical speculation. The interdict was issued against a gentleman who wished to alter his property, by supporting the front on cast-iron pillars, in the manner more commonly followed in this city in converting the ground-floor of dwelling-houses into shops. The front of the premises alluded to was narrow, and of a great height; and the reasons given for interdicting the operations were, that the pillars had not sufficient strength to support the weight of the front, and that the difference of the expansion of cast-iron and stone was so great, that very prejudicial effects might arise from the use of such pillars in this situation. Mr. Jardine, civil engineer, was desired to report to the Dean of Guild on the subject, and the proprietor requested my father to calculate the comparative expansibility of the cast-iron pillars and the stone, for a considerable alteration of temperature. Sufficient data were easily got for the rate of expansion of cast-iron; but the only experiments which could then be found on that of stone, are contained in a short notice by M. Destigny, in the 7th volume of the *Quarterly Journal of Science, Literature, and Art.* These are not extensive, and have, moreover, been determined by taking the difference between the expansion of the stones and rods of iron and copper to which they were attached, thereby diminishing quantities naturally very small; and the difference was again augmented and rendered more visible by a double system of levers: a method liable to many objections in point of accuracy, even though the experiments were performed with great care.

Since the date of the interdict formerly referred to, the almost universal adoption of cast-iron supports in constructing elegant fronts for

shops, and the perfect manner in which they have been found to answer the purpose, has given that practical refutation to the alleged objections which, in such cases, is always most to be relied on. And although I do not recollect that in any instance, a fire has taken place in a building, the front of which was supported on cast-iron, yet, from the experiments I have made, it will be seen that even under such circumstances the iron is not likely to be hurtful from the excess of its expansion over that of stone. Nevertheless it appears to have been a common opinion until within a very recent period, and one which has been supported by names of the very highest authority, that the length of a rod of Carrara marble could not be sensibly altered by a change of temperature. This opinion was founded on a very beautiful speculation on the arrangement of the crystals of marble; and although none of my experiments tend to show that it is correct with regard to either of the specimens of Carrara marble which I have examined, yet the supposition may not be altogether groundless, as I have found no stone which expands less than the black marble from Galway in Ireland.

Since the commencement of my experiments, I have seen a notice in the *London and Edinburgh Journal of Science*, of a letter read on the 12th of March, 1834, to the Geological Society of London, by Mr. Charles Babbage, in which the author states, that, from the experiments of Colonel Totten, recorded in Silliman's Journal, he has calculated a table of the expansion, in feet and decimal parts, of granite, marble, and sandstone, from which he finds the alteration in bulk so great, that, supposing the strata under the temple of Serapis to expand at the same rate as sandstone, and an increase of temperature equal to 100° to act on them to the depth of five miles, the temple would be raised 25 feet. He therefore concludes, that the different changes of level which this edifice has undergone, in reference to the sea, may be accounted for, simply by supposing the temperature of the subjacent rocks to have been altered. The sandstone which Colonel Totten had experimented on has been even more sensible to change than any of my specimens; because I find that a similar increase of temperature acting on a mass of what the marble cutters call Sicilian white marble, five miles thick, would produce an elevation of only 19.8 feet, or about $\frac{4}{5}$ths of the other; and it expands more than any other stone I have tried.

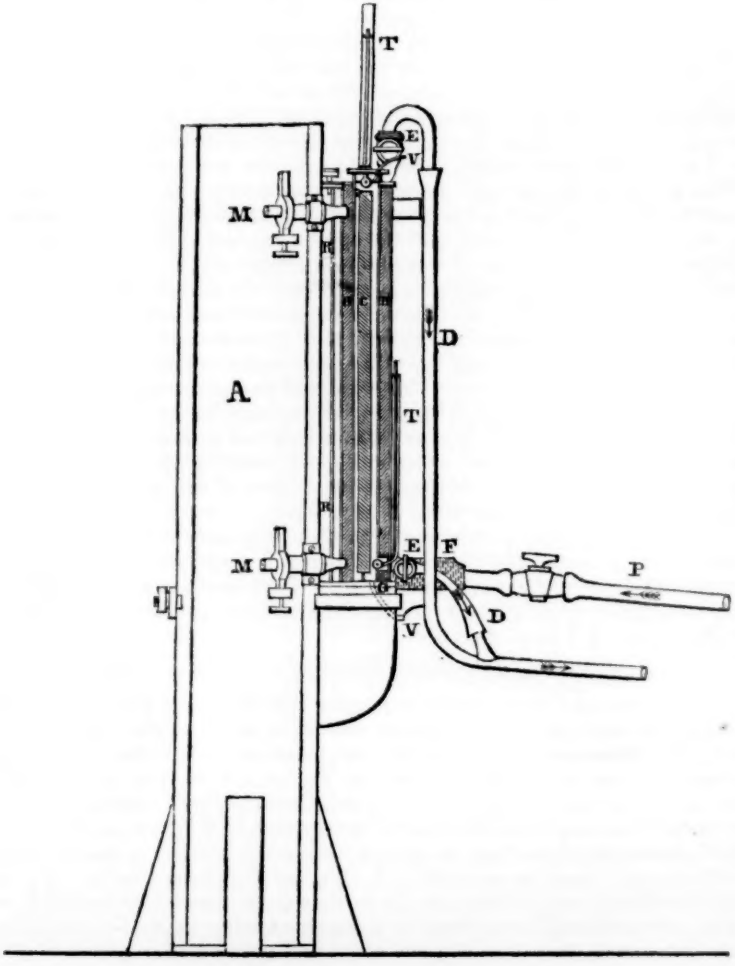
Before making any further remarks on the experiments themselves, I shall give an account of the construction of my pyrometer, and of the manner in which the experiments were conducted, so that every one may be enabled to judge how far he may rely on the accuracy of the results at which I have arrived.

Description of a Pyrometer heated by a current of steam.

The pyrometer I employed was constructed of a large piece of an oak tree of very straight growth, which was squared to eight inches and a half. In order to get the wood as well seasoned as possible, I selected it from the best of the oak beams got in the old houses lately pulled down in forwarding the improvements of Edinburgh; and from the number of coats of size paint which covered three sides of it, there could be no doubt that it had been long in an open situation. After it was worked to the proper shape, it was allowed to stand for some weeks, as it is sometimes said that, to remove the surface from timber, will cause it to warp, however well it may have been seasoned beforehand. The stand for this instrument, however, did not alter; and even smaller pieces of the

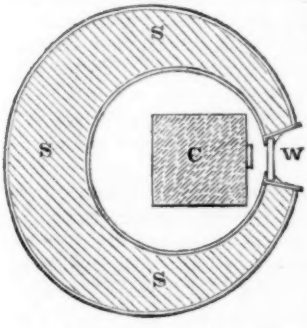
same tree, and of another tree from the same place, which were accurately fitted into other parts connected with the experiments, have not altered in the least degree; I therefore concluded that the wood had been very long kept in a dry situation. To the oak stand A, which was $4\frac{1}{2}$ feet long all the other parts of the instrument were attached. B is a double metal case containing the specimen C under experiment. The bottom of the case B rests in a strong brass plate, which is fixed on the top of a block of oak eight inches broad and eleven long, and the block is strongly hooped and bolted with iron to the stand. The upper part of the case B is fixed to the stand by a brass clamp. Fig. 2 is a cross section of the double case of the full size, and the part marked *s s* is the space through which the current of steam passes; so that the specimen C placed within the inner case, can be heated by the steam and yet kept

Fig. 1.



quite dry. P, Fig. 1, is the pipe by which the steam is brought from the boiler and thrown into the bottom of the double case, and D D are eduction pipes, from the top and bottom of the case B, by which the steam is carried off and thrown into the chimney, to prevent the atmosphere of the room from being rendered damp from its escaping into it. On each of the eduction pipes D D there is a stop-cock E, to regulate the quantity of steam which ought to pass off from the top and bottom of the double case, in order to keep the thermometers T T, which are inserted into the top and bottom of the inner case, at the same height; and by regulating the openings of these stop-cocks, the thermometers may easily be kept at the same temperature for any length of time. M M are the micrometer microscopes, strongly fixed to the stand A, and opposite to plate-glass windows W, Fig. 2, which are placed in the double case B, so as to suit the length of the rods of which it was wished to measure the expansion. The upper micrometer reads the thirty-thousandth part of an inch, and the under one was intended to have been used to read a much smaller quantity; but it required great care to draw the lines on the silver studs on which the lengths were laid off, so as to make the upper micrometer read to the same division; it was therefore found more convenient to employ the under micrometer as a fixed one, by moving the zero line of the under stud on the rod till it intersected the angle of the cross spider-threads in the under micrometer. This was done by the screw G working on the long end of a lever, which raised and lowered the rod under experiment; the lever, besides reducing the quickness of the screw G, gave it a more convenient position, and made the rod move without any tendency to turn. The whole body of the double case B was covered with a jacket about the third of an inch thick, made of several plies of flannel; and all the metal parts of the top and bottom, as well as the upper stop-cock E, and the greater part of the upper thermometer, were also thickly covered with flannel. This was found necessary to get the upper and under thermometer to stand at the same temperature. There was a screen R of polished tin-plate interposed between the stand supporting the micrometer microscopes and the double case B, in order to prevent any heat from being radiated from the one to the other. Also, in order to keep the stand of the pyrometer at the same temperature during the experiment, it was placed near the window of the room, and the furnace for the boiler was built in a common fire-place, the front of which was closed with a screen of thick boards; and the polished tin-pipes which conducted the steam from the boiler to the instrument and back again to the chimney, passed through holes in the screen-boards. A detached thermometer was hung over the instrument, and it was always kept at the same height by admitting more or less air by the window. In order to have the same direction of light during each experiment, I employed two small lamps, placed about a foot from the instrument, and their light was thrown in at the windows by lenses and reflectors. The reflectors were fixed on soft wires, so that they could be bent to reflect a bright light from the bot-

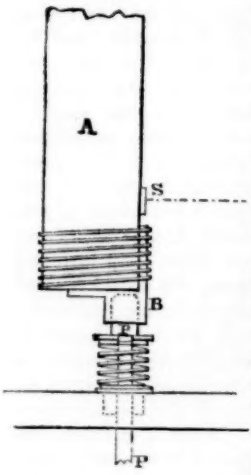
Fig. 2.



tom of the lines on the studs. All the parts of the instrument were made as heavy as consistent with convenience, to prevent their being affected by sudden changes of temperature.

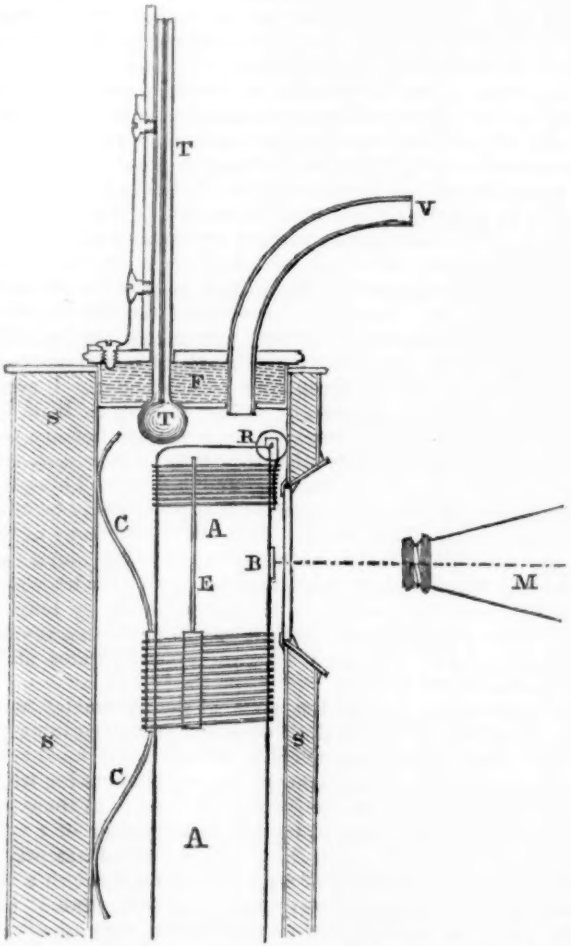
The rods of which the expansions were determined, varied from half an inch to an inch square, and the length of twenty-three inches was carefully laid off on silver studs, which were fixed into holes drilled in the rods, the centres of the holes being twenty-three inches apart. It was necessary to divide the head of the upper stud into fiftieths of an inch, in order to determine the value of the micrometer for each experiment, as it was liable to a little variation, from the impossibility of adjusting it to exactly the same focus which it had when it was adjusted to a fixed value. To guard against this error, arising from alteration of the focal distance, I read the micrometer for $\frac{3}{50}$ of an inch on the stud in the rod which was to be heated; and this was repeated at each experiment made on the rod, after every thing had been adjusted. In placing the rod in the instrument, a socket was tied to the lower end below the lower stud, and the socket was fitted so as to slip tightly upon the top of a square head, on the end of a pin P, Fig. 3, moved by the lever and screw G, Fig. 1. This served to raise and depress the rod, and was very convenient in fixing it in the proper place, for the under microscope. In Fig. 3, A is the lower end of the rod, S the under stud in it, B the socket, which is tied to A, by the piece on the front, so that it was of no consequence what the thickness of the rod might be as the stud was always in the same position in the instrument; *p* is the square pin fitting into the socket, and moved by the screw G, Fig. 1. The upper part of the rod was kept in the proper position, by means of slender springs of wire, which pressed a small roller against the front of the case, so that the upper stud always stood at the same distance from the object glass of the upper micrometer. This is shown by Fig. 4, which is a section of the upper part of the double case of the full size. A is the upper end of the rod under experiment. R the roller pressing on the front of the double case. S S the space through which the steam passes. B the upper stud opposite to the window and upper micrometer M; C C the back spring; E one of the side springs; F the cover of the top which is double; and the space between the upper and under plate is stuffed with cotton. T the bulb of the upper thermometer; V is the upper pipe, by which any vapour coming from the enclosed rod may escape, when it is wished to allow the rod to become drier; but the whole of the inner case is to be so tight as to prevent any evaporation, unless the upper pipe V, and the under one of the same size, V, Fig. 1, which passes through the bottom plate, are left open. The springs and roller represented in this figure were firmly tied on the rods, and could easily be shifted from the one to the other. It was my intention to have ascertained the expansion of the different rods between the freezing and the boiling points by first filling the space, S S, Fig. 4, with melting ice, and then by passing a current of steam to get the higher point, but the difficulty of getting ice until a few weeks since made me abandon

Fig. 3.



this, besides, I found that to keep the thermometers above 208° Fahr., sent a great heat through the room, on account of the strong fire it was necessary to keep up. As the thermometers were made with every precaution to insure their accuracy, I contented myself with the range I could get between the temperature of the room, which was generally under 50°, and 207° or 208°.

Fig. 4.



The rod was put into the pyrometer, the micrometers set, and allowed to stand over the night, and the lowest point taken at the height at which the thermometers stood next day, by making the line on the under stud intersect the angle of the spider-threads of the lower micrometer; and the index error of the line on the upper stud was read from the zero of the upper micrometer. This was repeated four times, by deranging and again adjusting the height of the under line by means of the screw G.

After completing the readings, the steam was turned into the case, and the expansion of the rod measured by a similar set of readings, as soon as the line on the upper stud was found to be stationary, which sometimes required the temperature of the pyrometer to be kept at the same degree for an hour and a half.

I thought that my application of steam for heating a pyrometer was the first that had been done, as I could find no account of its having been formerly used in any of the descriptions of the instruments for similar operations; but Lieutenant Murphy informed me, that he thinks it was employed in some pyrometric operations connected with the trigonometrical survey in India; and I am satisfied, from the uniformity of temperature it maintains in the instrument, no better method of heating can be resorted to for such operations, because the substance if of wood or stone is kept dry, and the fire or lamps are placed at such a distance as not to incommode the operator in the least degree. Some of the rods of stone experimented on are now before the Society. I had considerable difficulty in making rods of Roman cement, as they repeatedly cracked in drying, and I was anxious to have this substance tried, as I found that rods of lime were too weak, not more to settle the general question, as to the expansion of buildings, than to have the pleasure, if possible, of assisting Mr. Brunel in accounting for the results of some experiments he mentioned at the last meeting of the British Association, when describing one of the many beautiful contrivances for which his fertile ingenuity has rendered him so celebrated. I succeeded in making a rod of Roman cement, by at first mixing as little water with the powder as would make it work, and when it had become too plastic, from continued working, more of the powder was added, and the cement again made plastic by working and beating. This process was continued so long as any of the cement in powder could be added to the mass, and the whole again worked into the consistency of soft putty without any addition of water, as the strength of the mortar appears to depend on the smallness of the quantity of water with which we can make the dry powder plastic, this is accomplished by long continued working. After preparing the mortar, it was put into a mould of Bristol board, and when it had set, it was immersed in water, and left for a fortnight to harden.

Having ascertained that all the parts of the pyrometer worked well from the uniformity of the results I obtained in determining the expansion of cast zinc, which was selected as the metal most sensible to a change of temperature, I commenced the experiments for which the instrument was made; but before giving the results of those on stone, I may mention what took place in trying the expansion of a rod of oak, cut from the same tree of which the pyrometer stand was made. On account of a very small quantity of steam escaping through a hole in the inner case, the wood expanded very much the first time it was heated, and when taken out, and allowed to stand some days to dry, I found it lengthened about the thirtieth part of an inch. The inner case was made perfectly air-tight, even under considerable pressure, as it appeared absolutely necessary to prevent steam from escaping into it, for all experiments to ascertain the effect of heat on different kinds of wood; and I also wished to keep the rods of stone quite dry, to be certain what effect moisture had on their expansibility. When the rod of oak was again thoroughly dried, and the length of 23 inches laid off anew on the studs,

it was found that for an increase of temperature of 180° Fahr. it only expanded .001426177 of an inch, or .000062007 in decimals of the length of the rod; which is just one-fifteenth part of the amount of the expansion of platinum, the least expansible of the metals. This insensibility to change of temperature in this wood, provided it be kept free from moisture, has induced me to investigate the subject a little farther. I have procured rods of many of the most straight-grained kinds of wood, and after I have determined their rates of expansion, I intend to varnish or cover them with different substances, to find their expansion again, and then to see to what extent, and for how long, they will resist moisture as it has been said that a wooden rod answers best for a pendulum when unvarnished, and left in its natural state.

Table of the Expansion of Stone, &c.

	Decimal of an inch on 23 inches for 180° F.	Decimal of length for 180° F.	Decimal of length for 1° F.	
1. Roman cement,	.0330043	.0014349	.0000075	When the rod contained more moisture it expanded more.
2. Sicilian white marble,	.0325392	.0014147	.0000078	From 1st experiment when moist.
	.0253946	.00110411	.00000613	Mean of three experiments when dry.
3. Carrara Marble,	.0274344	.0011928	.00000662	From 1st experiment, when moist.
	.0150405	.0006539	.00000363	Mean of two experiments, when dry.
4. Sandstone from the liver rock of Craigleith Quarry,	.0270093	.0011743	.00000652	Mean of four do.
5. Cast iron from a rod cut from a bar cast two inches sq.	.0263755	.00114676	.00000637	Mean of two do.
6. Cast iron from a rod cast half inch square.	.0253498	.001102166	.00000612	Mean of two do.
7. Slate from Penrhyn Quarry, Wales	.0238659	.0010376	.00000576	Mean of three do.
8. Peterhead red granite,	.0220416	.0009583	.00000532	From 1st experiment when moist.
	.0206266	.0008968	.00000498	Mean of two do. when dry.
9. Arbroath pavement.	.0206652	.0008985	.00000499	Mean of four do.
10. Caithness pavement,	.0205788	.0008947	.00000497	Mean of three do.
11. Greenstone from Ratho,	.0186043	.0008089	.00000449	Mean of three do.
12. Aberdeen gray granite,	.01815695	.00078943	.00000438	Mean of two do.
13. Best stock brick,	.0126542	.0005502	.00000306	Mean of two do.
14. Fire brick,	.0113334	.0004928	.00000274	Mean of two do.
15. Stalk of a Dutch tobacco pipe,	.0105177	.0004573	.00000254	Mean of three do.
16. Round rod of Wedgewood ware (11 inches long,)		.00045294	.00000251	Mean of two do.
17. Black marble from Galway, Ireland,	.0102394	.00044519	.00000247	Mean of three do.

The preceding Table gives the expansions of the different kinds of stone I have examined, and also of two rods of cast-iron. The one which expanded least, was cast half an inch square, and was put into the pyrometer with the outer surface rough as it came from the mould. The other rod was also half an inch square, but was cut out of the centre of a bar of the same iron cast two inches square, in order to get more nearly the rate of expansion of a large mass.

In the Table, the substances are arranged in the order of their rates of expansion, that which expands most being placed first.

From the results given in the Table, it is evident that no danger is to be apprehended from a change of temperature affecting cast-iron and sand-stone, in a very different degree, as their expansion, in so far as regards all building operations at least may be considered as the same.

The difference between the expansion of bricks and malleable iron amounts only to .00042 for 98° Fah., or half an inch on 100 feet; and Roman cement, when in as damp a state as all building must be, will expand more than malleable iron, since it did so in my experiment, even after it had been dried for eleven hours at a temperature of 207°. This, therefore, will in a great measure serve for an explanation of the fact mentioned by Mr. Brunel, in describing his method of constructing arches, by suspending the courses of brick with straps of hoop-iron, viz., that he had some anxiety as to the manner in which variations of temperature would affect his mode of operating, by expanding the metal more than the brick and mortar; but, on examining his experimental arch, both in summer and in winter, not a crack was to be seen. The substances at the bottom of the Table are black marble, and the rod of Wedgewood's ware; their expansions are nearly alike, and they expand very nearly half as much as platinum for the same number of degrees.*

M. Destigny mentions in his paper, before alluded to, that he found the expansion of stone not at all affected by a difference in its state of humidity. From what I have observed in conducting my experiments, I am quite of a different opinion, for although no increased expansibility was observed by wetting sand-stone after it had been dried, and I did not try any direct experiment of this nature with any of the marbles, on account of the very small quantity of water which they were found to absorb after having been dried, nevertheless the experiments on greenstone show the effect of moisture, and some of the marbles certainly became less sensible to the change of temperature, after they had been heated, and this was probably caused by their natural moisture being driven off. This diminution is distinctly shown by the following series of experiments on Sicilian marble:

1*st*,	.0325392	Diff. .0058229	Decimals of an inch for 23 inches, the change of temperature being 180° F.
2*d*,	.0267163	.0013143	
3*d*,	.0254020	.0013364	
4*th*,	.0240656		

The rod gave off a considerable quantity of moisture. This gradual diminution of the expansion was scarcely observable in the sand-stones and Aberdeen gray granite, which contained a great deal of quartz; and in the bricks, in the stalk of a Dutch tobacco-pipe, and in a rod of Wedgewood's ware, no such change took place, although all these sub-

* I have since procured another rod of Galway black marble, which contained more ossils, and was softer than the first. Its expansion for 180° Fahr. was .0004793, and for 1° Fahr. .00000266.

stances had been wet in cutting the holes, &c. before they could be put into the pyrometer. The Peterhead granite, again, which contains more feldspar than that from Aberdeen, and also the green-stone, were affected by losing their moisture, but more especially the latter, as shown by the following series of experiments made in November. Unluckily the first observation, which always differs most from the others, was lost.

2*d*,	.0198379	Diff. .0017485	Dicimals of an inch, the expansion of 23 inches for 180° Fahr.
3*d*,	.0180884	.0002037	
4*th*,	.0178857		

As greenstone absorbs moisture much more readily than the marbles, I replaced the rod again in the pyrometer, after it had hung in the room for more than three months, from the time of performing the first experiments. The instrument was heated to 207° Fahr., and this temperature was kept up without change during the four hours in which I watched the alterations of the length of the rod. The inner case being closed to prevent the evaporation of the moisture, the ultimate expansion of the green-stone for 155° F. was 5.02 revolutions of the screw of the upper micrometer. The pipes V V, for drying the inner case were then opened, a good deal of vapour came off, and when the rod had been allowed to dry for three-quarters of an hour, the micrometer read only 3.24; hence by the effect of drying alone, the rod contracted rather more than $\frac{1}{170}$th part of an inch; I then poured boiling water on the rod, and closed the drying pipes, and although this could not moisten it thoroughly, yet when its expansion was again at the greatest, the upper micrometer read 4.15. The drying pipes were then opened a second time, and although the temperature always remained unaltered, the reading of the upper micrometer was again reduced to 3.32. This experiment clearly shows, that the expansibility of some kinds of stone very much depends on their state of humidity. An analogous experiment was tried with a rod of satin wood, and the length of the wood altered with the similar changes in the same manner, but to a greater extent. With the latter substance this might have been expected, but such an action of moisture on stone is certainly very remarkable. I have therefore given two rates of expansion in the table for several of the rods, the greater is when the rod contained most moisture, the lesser is a mean of several experiments made when it was in a dry state, and when the results of the experiments agreed very nearly among themselves.

A very curious effect of the heat on the white marbles, was that of causing a permanent increase of their length, which, in the Sicilian marble was gradually augmented by the heating for each experiment. The rod of this marble was heated five times, but, unfortunately, as the change is always decreasing, I at first adjusted the micrometers without noting the quantity, by which the line on the stud had not returned to the adjustment for the previous experiment. This was the first time I had met with this gradual increase in the length of the rod; and by the effect of the two heatings noted after I observed it, the rod was lengthened .0096, or nearly the hundredth of an inch, on the rod of Carrara marble, the elongation took place mostly at the first heating, and it amounted .0135 of an inch. This may probably serve to account for the warping which sometimes takes place on those parts of chimney-pieces which are subjected to the greatest action of the fire. It is impossible to say how far this lengthening would be increased by a greater change of temperature; but, since Mr. Babbage has applied the expansion of stone

to the explanation of geological phenomena, this property of limestone may also be taken into account, as it only requires the heat to have been once in action after the strata have been formed. In the experiment on the rod of Carrara marble, the whole effect was produced by an increase of temperature of 157° F.; and if we suppose this to have acted on strata five miles deep, the depth taken by Mr. Babbage, in accounting for the changes of the level of the Temple of Serapis, the surface would have been raised by not less than 15½ feet. Little weight, however, can be attached to such explanations of the great operations of nature, as there may be a vast difference between the effect of heat quickly communicated to the small pieces of stone on which we perform our experiments, and the necessarily slow diffusion of the heat through such a depth of strata as we have supposed.

In order to ascertain if there was any connexion between the density of stones of the same composition and their expansibility, as takes place with the metals, I determined the specific gravity of several of the rods, but the one property does not appear to have the least dependence on the other. For instance, the specific gravity of Sicilian white marble and Galway black marble is 2.7127 and 2.7093, almost exactly alike; the expansion for 180° F. of the former is .0325392, while that of the latter is only .0102394, not one-third of the first. In determining the specific gravities, the stones were first dried on a sand bath, and, after being weighed in a dry state, they were put into distilled water; and although the pieces of the black and white marble weighed about 200 grains, and were left three weeks in water, they only absorbed one-tenth of a grain, yet the white marble, in particular, gave off a considerable quantity of moisture when first heated. The Arbroath pavement absorbed $\frac{1}{28}$ of its bulk of water, and the Caithness pavement $\frac{1}{247}$ part; which shows the great superiority of the latter pavement for all purposes, where it is wished to exclude dampness.

In conclusion, it may be observed that, from the results of the experiments given in the Table, it is perfectly evident that not the slightest danger can arise from the use of cast-iron in buildings, on account of the difference of their expansion for all ordinary temperatures. Nor do I think that there is any cause to dread the effects of fire, where such pillars are used, although that was one of the arguments employed to retard their introduction; on the contrary, there is every reason to believe, that even in cases where the fire would rage with all the fury that a strong wind could impart to it, cast-iron pillars would support the lintels of the windows as safely as those of stone. And I do not state this as a mere matter of speculation. The late great burning of the North Bridge New Buildings, is a proof of the correctness of part of the opinion. Of the extraordinary fierceness of this conflagration, those who did not witness it might easily have satisfied themselves, by inspecting the injury done to the stones of the ashler front. I was present at this terrific scene of destruction, before the flames burst from the windows, and remained till the fire was quite under the power of the engines. Very soon after the flames came sweeping out at the front window, the stones began to crack and skirt off, and a constant shower of such large masses fell, during probably half an hour, that the firemen were obliged to draw back their leathern hose to a considerable distance from the buildings; and, so long as the stones continued to fall, it was impossible to save any of the very valuable property from the shops

below, which, but for this might have been easily and leisurely done. Now, no part of the stone was at any time red hot, and the firemen were standing on the window-soles very soon after the flames retired from them, so that no danger was to be apprehended from the risk of cast-iron being melted in such a situation. Indeed, the cast-iron columns in the interior of the building did not appear to be much injured by the fire.

Edinburgh, April 4, 1835. Trans. Royal Soc. of Edin.

First Russian Railroad, from St. Petersburg to Zarscoe-Selo and Pawlowsk, established by Imperial Decree of 21*st March*, 1836, *and carried into execution by a Company of Shareholders in Russia, England, and Germany.* Translated from the German. St. Petersburg, 1837.

The following brief analysis, and extracts (taken from the London Mining Journal and commercial Gazette) of the first attempts at Rail Road making in the Russian Empire, cannot fail to interest the readers of this Journal. The extraordinary skill and energy of the Chevalier von Gerstner, to whom the execution of the whole scheme was confided, and the no less extraordinary rapidity and success with which the work appears to have been accomplished; must challenge the admiration of all who feel an interest (and who does not) in improvements of this nature wherever they may be effected. G.

In our former notice of this pamphlet we gave a brief history of the origin of the railway now forming, and indeed nearly completed, from St. Petersburg to Zarscoe-Selo and Pawlowsk, being the first work of the kind in the Russian empire, and probably only the precursor of others of far greater extent and commercial importance. We pointed out the extraordinary privileges with which the company was endowed, and the spirited mode in which the undertaking was commenced and carried on, under difficulties of some magnitude as regards the supply of the necessary materials and machinery.

We now return to the work, chiefly for the sake of making such extracts as are likely to prove interesting to our readers, as showing the spirit and activity which prevails in that vast empire, which, both politically and commercially, must be an object of no inconsiderable interest to all the other nations of Europe, and of whose advances in the development of her mineral wealth, we lately furnished a notice of considerable length. We shall first quote the introductory pages, omitting merely some tabular statements, as being of secondary importance:—

"The reports on the Zarscoe-Selo Railroad, which from time to time have appeared in the Russian papers, and from thence have been copied into the foreign journals, have excited considerable interest in the public, particularly in Germany and England. No undertaking of the same class has hitherto made such rapid progress as this railway, which, called into existence by the especial patronage of the Emperor of Russia, and endowed with most extensive privileges, precludes all doubt of its proving eminently successful and advantageous to the shareholders. In a few days subsequent to the Imperial grant being obtained, the company of shareholders was formed, and the whole capital (three millions of rubles) subscribed. The payments of the calls upon all the shares issued, were made with punc-

tuality, and no single instalment remained in arrear. Within six month, and six days from the time the decree received the imperial sign-manual, the requisite supply of rails and materials, carriages and machinery, was obtained from England; most of the works on the entire length of lines twenty-five and a half wersts, were completed, the rails partly laid down, and three wersts opened by horse power. Six weeks later the opening of a distance of seven and a half wersts, with locomotives, took place; and this summer (1837) the whole of the line, from the centre of the capital to the terminus in Pawlowsk, will be opened.

"This rapid progress of the undertaking, which in other countries would have been the work of several years, naturally excited the attention both of natives and foreigners.

"That portion of the English public which takes a general interest in railways, expressed a desire to obtain the reports which have hitherto appeared, that they might be enabled to investigate the circumstances that led to so extraordinary a result in Russia; another portion, better acquainted with the favourable state of the Russian share market, with a view to partake in this new speculation:—but as the publications of the Chevalier von Gerstner—viz:—

"*Memoir on the advantages of a Railroad from St. Petersburg to Zarscoe Selo and Pawlowsk, 20th March*, 1836.

"*First report on the progress of this Railroad, 20th July*, 1836.

"*Second report on the same, 22nd September*, 1836; have for some time been out of print, the

"*Third report, 29th January*, 1837, which has reference to the preceding, would hardly be understood by many readers. The purport of the present paper is, therefore, to lay before the public in this country a clear statement of facts relative to the Zarscoe-Selo railway, to assist them in forming a correct opinion of the enterprise, and in entering into it, as well as into other manufacturing concerns about to be carried into effect in Russia. It will tend, at the same time, to give a more expanded view of the internal constitution of that colossal empire, and to correct the erroneous opinions that have been induced by a defective knowledge of its actual condition.'

"Peter the Great, the immortal founder of the power and greatness of the Russian empire, felt how necessary to its welfare was the improvement of its communications; he had witnessed in Holland the beneficial influence of canal traffic; he visited the interior of his empire, and himself planned the whole of the water communications which were, either during his reign or afterwards, carried into execution. This sovereign introduced *Canals* because, in the then existing state of knowledge, they were considered as the most perfect channels of internal communication. Alexander I. introduced artificial *Roads*. He commenced with the first turnpike road from St. Petersburg to Moscow, a distance of about 700 wersts, the smallest portion of which only was accomplished when death overtook this monarch; but his successor, the reigning Emperor Nicholas I., carried out the project; in a few years finished the road to Moscow; and caused surveys to be made for a complete system of roads, intended to intersect the whole interior of the empire, which, under his happy reign, is now making such rapid advances in prosperity.

"The progress which *Railroads* had made in modern times did not escape the scrutinizing view of the sovereign. The Chevalier von Gerstner went to Russia in August, 1834, with the intention of visiting the interior of the

country, and informing himself respecting its manufactories and mines. The Emperor NICHOLAS heard of this, as well as that so early as 7th September, 1824, the Chevalier had obtained a privilege from the late Emperor FRANCIS of Austria, to construct a railroad between the Moldau and Danube; that for four years, up to 1828, he conducted the works on this line, comprising, with its continuation from Linz to Gmunden, a total length of 130 English miles, over which the traffic continues in summer and winter without interruption. The Emperor in consequence, in September, 1834, expressed a desire to see a line of railway from St. Petersburg to Moscow executed, if possible, by a company of shareholders.

"After the Chevalier von Gerstner had in the beginning of 1885, finished his tour to the manufacturing provinces, he was presented at the Court of St. Petersburg, when the Emperor, with great earnestness, and with the penetration for which he is so remarkable, expressed himself strongly as to the advantages that would result from the introduction of railways into Russia, and the extraordinary privileges that the first undertakers might expect.

"The Chevalier von Gerstner in consequence proposed to commence with two short lines, the first from the interior of St. Petersburg to the towns of Zarscoe-Selo and Pawlowsk, and the second from the same point in the capital to Peterhoff and Oranienbaum.

"The negociations for the grant lasted to the end of the year 1835, when, on the 21st December, the President of the Council, in the name of the Emperor, communicated to M. von Gerstner that he was thenceforth invested with the *exclusive* personal privilege of *incorporating shareholders for the execution of both railways.*

"The Chevalier von Gerstner hereupon joined the three other directors and founders of the company of shareholders, whose names have been given above, for the execution of the first line, from St. Petersburg to Zarscoe-Selo and Pawlowsk, whilst he reserves to himself the right of forming the company for the line to Peterhoff.

"The demand for shares, on the undertaking of the enterprize being made public, was so great that the first 15,000 shares, or the original capital of three millions rubles, were subscribed for almost immediateiy; chiefly by the Russians and naturalized Germans, although persons residing abroad may participate, without any restriction. The latter may either appear in person at the general meetings, or be represented by their agents, and receive their dividends the same as the Russian subjects, without any deductions or the payment of any duties to the state.

"The Imperial Grant for the Zarscoe-Selo railway is dated 21st March, 1836. The privileges thereby conceded to the company are very considerable, such as were never granted to any railway company in any country before. The execution of the railway is regarded as if undertaken immediately by the Crown; the Crown lands have been gratuitously ceded to the company; the farmers holding lands that were required, have been appointed to other ground, and are compensated by the Crown for any loss sustained by the transfer; lands or buildings, the property of private individuals, must be surrendered to the company, either by voluntary agreement or at a price to be determined by judicial valuation; but to prevent the obstruction of the works, the company, by depositing a sum of money about equal to the purchase price of a similar plot of ground or tenement in the vicinity, have the power to take possession of such lands or tenements before the termination of the appraisement. The valuation being determin-

ed, the balance of the amount due is paid to, or received from, the parties.

"The company are at liberty to erect any description of buildings requisite for the railway traffic, for 100 fathoms on each side of the railway, except in the Artillery Gound, through which the line passes, and for the acquisition of which the same privileges have been granted as for that of the rest of the line. The removal of the battery, rocket manufactory, and other military buildings intersected by the line, is to be effected by the company, at their cost, to another quarter. In this manner the railway without the town forms an uninterrupted straight line for twenty-four wersts. Within the town the straight line is only warped into two gentle curves, by following the course of the Wedenskoi canal.

"The medium rise of the whole line is one in 1028, and the extreme one in 504. The railway terminus in the town is at the junction of the Wedenskoi with the Fontanka canal, on a piece of ground eighty fathoms by forty-two and a half in breadth, which has partly been purchased and partly ceded by the Crown. From this spot to the new boundary of the town on the Ligofka, the railway measures about one and a half mile, and will, therefore, for that distance, *run within the capital*, a circumstance of the utmost importance, as effecting the number of passengers or the amount of traffic. The terminus at the other end is situated 550 fathoms within the Great Park of Pawlowsk the property of his Imperial Highness the Grand Duke Michael Pawlowitsch. The company have been permitted to erect, in some of the finest parts of that park, several buildings for the reception and entertainment of the public; and at Zarsco-Selo they have been allowed to establish an hotel at the railway station.

"The company have the right to purchase the iron for the whole line, abroad, and to import the same *duty free*, provided no Russia iron worker should undertake the delivery in the required quality, form, and time, and at most at fifteen per cent. advance upon the price at which the iron might be imported into St. Petersburg, from foreign countries. The company are further empowered to import the locomotive engines, railway carriages, and all other machinery and requirements, *duty free*.

"The company are not bound to any fixed fares for the conveyance of passengers, or rates for the carriage of goods, but are at liberty to fix them at discretion. The railway remains *for ever* the property of the company: during the first ten years no one can make a railroad in the same direction; and during the same term of ten years the company are exempted from the payment of rates and taxes of every description; either to the Post Administration or any other authority. The capital of the company is covered by 15,000 shares of 200 bank note rubles each; if necessary, 2500 more shares may be issued for raising the reserve fund of 500,000 rubles; all the 17,500 shares, however, participate alike in the profits of the undertaking. *The number of shares can in no case be augmented.*

"Eight days after the grant had been made out, the Chevalier set out from St. Petersburg for England and Belgium, for the purpose of ordering the necessary rails, engines, carriages, and other railway machinery, as no one could be found in Russia to contract for these materials in the stipulated time.

"It was exceedingly difficult to obtain them in England last year, as the iron works there, in consequence of the many English and American orders, were occupied literally night and day, and most of them had employment for a year or two in advance, in consequence of which the price of rail bars

had been raised upwards of forty per cent. within a twelvemonth. Another difficulty arose from the Chevalier having altered the *Gage* (or the distance of the two rails upon which the carriages run) of the Russian railroad, from that established in England. On the old English railroads only goods of small bulk and great weight were transported, such as iron, coals, stones, bricks, &c., but not sheep's wool, hay, straw, or fire-wood. In 1822 when the railway between Stockton and Darlington was begun, which was first intended for a general traffic of passengers and goods, Mr. George Stephenson, the engineer, established the breadth of the track between the rail at four feet eight and a half inches English, as being the width of the track of carriage-wheels on high roads. Experience has shown how inconvenient this arrangement is for the locomotive engines, which in England, usually of thirty to thirty-five horses' power, are by this narrow gage confined within about four feet, which is by far too little for such an engine. The driving wheel can at most have but a diameter of five feet, as otherwise it would lurch too much; in order, therefore, to do thirty miles an hour, it must make 168 revolutions in a minute. The strain and wear and tear of all the parts of a locomotive, by reason of the quick motion of the driving wheel, and more particularly the cramped arrangement of the individual parts, are therefore very considerable.

"The disbursements in the coaching and carrying departments on the Liverpool and Manchester railway, upon an average of the last three years, amounted to forty-four per cent., and the repairs of the locomotives to fifty-six per cent. of the total expenditure, originating in their wear and tear as a tractive power. This charge must naturally be far less in a wider gage. If cattle and bulky materials, such as sheeps' wool, straw, hay, fire-wood, travelling carriages, &c., are to be conveyed, the load cannot be stowed between the wheels, if the gage is only four feet eight and a half inches, but must be placed in a box or on a platform six to eight feet wide, by which the base being only half as large as the superstructure, great lurching is necessarily occasioned, particularly at high velocities; and moreover, especially in rough weather, high loaded waggons, with a confined base, are apt to have the flange of the wheels rub up against the rails, thereby occasioning great increase of friction, wear and tear. The trains generally run the distance between Manchester and Liverpool in one and a quarter to one and a half hour, whereas in a high wind those laden with cotton or wool take three hours for the journey, which would not be the case if a wider gage had been adopted. Increase of axle-friction cannot take place on a wider gage, as in all railway carriages the friction is now no longer between the wheels, but on the outside, on the projection of the axle through the wheel.

"These and other reasons induced the Chevalier von Gerstner to *adopt a gage of six feet English between the rails;* but the consequence was, that for the locomotive engines, turnplates, and machinery, new drawings and models had to be prepared, before the construction of the machinery, could be commenced. The deliveries thereof last year were in consequence attended with considerable difficulty; but connexions of many years standing which the Chevalier von Gerstner had in England, enabled him to overcome these difficulties, and in the Third Report there occurs an account of the rails, chairs, &c. imported duty free into Petersburg."

Passing over some details of the work, and the partial opening of the railway, from which it appears that it progressed satisfactorily, and that

the snow occasioned no impediment on the line, we come to the following extract from the Chevalier von Gerstner's third report :—

"Whoever will bear in mind that the Imperial Decree was dated on the 21st March, 1836—that until then I could not take my departure for Belgium and England, for the purpose of giving the orders in those countries for the railway materials—that these orders came when the English manufactories and iron works were occupied night and day, to the utmost stretch of their capabilities; whoever will bear in mind that the gage of the English railways is fifty six and a half inches, whilst ours is seventy-two inches, and therefore, that not a locomotive, not a carriage or a turntable could be purchased and imported that had not to be planned and have new models expressly made for it, after which, and not until then, the various machinery could be put in hand and made; whoever will further consider that every rail and piece of machinery, when finished, had to be conveyed to some sea port town, there to be shipped for Cronstadt; on its arrival there, to be transhipped into lighters, to be brought on the Fontanka canal to St. Petersburg, again unloaded, and, frequently on the worst of cross roads and marshes, carried to the point of their destination on the railway, there to be laid down and adjusted or fitted up; whoever will calmly examine and weigh all these matters, will not withhold from the direction of the railway of which I have the honour to be a member, the testimony that nothing but the greatest ardour and spirit, systematic management and punctuality, especially in the payments, could, in so short a time, have brought about a result, which in other countries, it generally takes years to achieve.

"As by the arrangement which the directors concluded with me in the name and on behalf of the company, and by virtue of section 10 of the Imperial Ordonance, the works were committed to my management under certain conditions, involving great responsibility, I lost no time in applying all my energies to the promotion of this great undertaking.

"Although I personally directed the works along the entire line, and traversed it continually on foot from St. Petersburg to Pawlowsk and back again, I nevertheless thought it right to employ seventeen engineers, five of whom had visited the railways in England, as superintendents; besides these, upwards of thirty inspectors, and as many watchmen and soldiers were periodically present on the line. For erecting the machinery and as enginemen for the locomotives, ten Englishmen and two Belgians came over—of the former five have returned.

"Solidity of materials and accuracy of workmanship,—strict integrity in all money transactions;—the greatest publicity in the accounts, —indefatigable attention to the progress of the undertaking,—system and order in the management, were the points I kept in view from the moment in which I commenced this enterprize. By the observance of them I have brought the work, so far as regards my own department, to its present state; by similar conduct I shall bring it to an honourable issue. Russians, Germans, Englishmen, Italians and Belgians, co-operated in our undertaking, and a stranger might not unreasonably have been astonished when, on the point of the line at the building at Pawlowsk he heard five languages spoken and found the people of five different nations here, at so distant a point of Europe, unite for one purpose—the promotion of our great object.

"I consider it my duty to state, and do so with pleasure, that the Rus-

sian population, so far from throwing obstacles in our way, have invariably been anxious to assist in the execution of the work.

"Incredible as it may appear to my foreign readers, I can state as a fact that no opposition has been evinced (as has almost invariably been the case in other countries) to so great an innovation. The proof is easily adduced, for not a single complaint of premeditated obstruction has come before the authorities. Our difficulties hitherto have been confined to the negociations with the proprietors of the plot of ground between the Ligofka and Fontanka, being town lands; without the town the farmers freely ceded their lands; on the first intimation being given to them by the surveyors, they cleared their forests to the width of sixty fathoms—nay, they mowed the half ripe corn in their fields, in confident reliance that the compensation the law has awarded them would be duly paid. The peasantry from the whole surrounding country worked on the line; and they were the first, who upon its opening crowded into our railway office, there paid their forty kopeks for a seat and availed themselves of the new conveyance. The coachmen and inkeepers at Zarsco-Selo and Pawlowsk, immediately perceived the prospect of gain which was opened to them; the former arguing the increase of their business in the conveyance of such a number of persons to the stations, and the latter that they will have to accommodate crowds of visiters whom the company's buildings will be insufficient to contain."

Our concluding extract will furnish some account of the object of the Zarscoe-Selo Railway, and the estimated amount of traffic and of profit to the shareholders:—

"St. Petersburg, the modern capital of the Russian empire, was founded by Peter the Great in 1703, at the entrance of the Neva into the Gulf of Finland, in a low, and formerly quite a marshy country. However advantageous this site may be for trade, it has been attended with incalculable disadvantages; amongst which may be reckoned the periodical inundations, of which the 7th November, 1824, afforded so dreadful an example; the changeable state of the weather and temperature, the fogs which envelop the town, the difficulty of constructing sewers for carrying off the impurities from the streets, and of laying down pipes for spring water. The canals which intersect the town are no doubt of importance for the conveyance of fuel and building materials to the town; but they likewise serve as receptacles for filth, and in the heat of summer fill the air with offensive effluvia. The sewers of the principal streets, for want of sufficient fall are at intervals obliged to be opened, and the contents laid in the streets and carted away.

"The consequence is, that the exhalations, especially in warm weather, are very unwholesome, and hence the urgent necessity that the inhabitants of St. Petersburg should, at that season, leave town to reside in the country.

"Hitherto the country houses have been confined to the adjacent islands, elevated only a few feet above the Neva, and in spring and autumn, therefore, covered with fogs. These islands, nevertheless, were generally filled with temporary residents, and country houses continued to spring up in the immediate vicinity of Petersburg. The cheerful town of Zarscoe-Selo is sixteen English miles from St. Petersburg, situated on an eminence bordered by the Neva. The imperial palace there

is 220 feet higher than the Neva; the town of Pawlowsk, two and a quarter English miles further, lies 105 above the Neva. The charming situation of these two places, and of the imperial parks adjoining, are sufficiently acknowledged and admired; but owing to the distance from the capital, not above 300 families could annually reside there in summer. Notwithstanding the distance, however, both were much frequented on Sundays.

"For a conveyance to Zarscoe-Selo or Pawlowsk, 18s. to 22s. sterling is paid; for a place in the stage coach three and a half rubles, and as much back; nevertheless from inquiries instituted by government in 1834, the number of equipages passing from St. Petersburg to Zarscoe-Selo and Pawlowsk and back again, for one year, was 70,386, employing 178,187 horses. At the rate of one passenger for each horse, the present annual transit of persons would thus be 178,187, of whom half or 89,093 make the journey there and as many back. This great traffic may be attributed to the delightful situations of the towns, as well as to the circumstance that in Zarscoe-Selo and Pawlowsk five regiments, with 300 officers, are stationed, including the two depot regiments, one of infantry, and one of cavalry, the officers of which change every year or two, as they only learn the routine of duty here, and then return to their stations in the interior of the empire; the traffic therefore on the part of the officers is the same winter and summer.

"From the perfect construction of this railway, sixteen miles of the line being straight, with a medium rise of one in 1028 the whole journey to Zarscoe-Selo will be performed in thirty-five minutes, and to Pawlowsk in forty-minutes. Both places will therefore be converted into suburbs of St. Petersburg, as the time alone fixes the distance. If a passenger is forwarded by the railway for about one and a half rubles, there can be no doubt that as the line runs one and a half miles from the Ligofka to the Fontanka Canal in the interior of the town, a great traffic may be calculated upon.

"In Belgium the railways commence without the towns; in order therefore, to get from Brussels to Malines, it is necessary to ride in an omnibus from the interior of the town to the railway station without and there await the starting of the train, which certainly in thirty or forty minutes arrives at the Malines station; but that being also without the town, it is necessary to get into another omnibus to drive into the town, and in this manner as much time is taken up as by the journey on the railway itself.

"On the St. Petersburg railroad the starting place is nearly in the centre of the city, and it penetrates into the heart of the park of Pawlowsk where the buildings for the reception of the public, stand. As on the railway from Brussels by Malines to Antwerp, upwards of 100,000 passengers pass monthly, and as is well known not one-fourth of them travel on business, the rest for pleasure and recreation, a very considerable traffic must also be expected on the Petersburg line. Nevertheless the Chevalier von Gerstner took in his first estimate of 20th March last year, the number of passengers who would annually travel to Zarscoe-Selo and Pawlowsk and back again, only at 300,000.

"The population of St. Petersburg, Zarscoe-Selo, and Pawlowsk amounts to half a million, that of Brussels, Malines, and Antwerp is not half so much; there we have 1,200,000 travellers annually, here we only

calculate upon 600,000, who are to make the journey once; or, in proportion to the population, only one-fourth part of the passengers on the Belgian lines. At this rate, supposing the whole 17,500 shares were issued, there would result a net profit of eleven per cent. to the holders."

The remaining portion of the work treats of a proposed extension of the railway to Ischora, the first station on the road to Moscow, being a preparatory measure to a railway connecting the modern with the ancient capital of Russia—certainly a most gigantic undertaking. It also contains certain terms on which the Chevalier von Gerstner offers to farm the Zarscoe-Selo Railway for three years—a proposition showing the strong confidence he entertains in the remunerative results which will arise from it. Lond. Min. Jour. & Com. Gaz.

Mechanics' Register.

¶ *Description of Mr. Cocking's Parachute and a narrative of the circumstances attending his fatal descent.*

Considerable interest existing at the present moment on the subject of aërial navigation—an interest which has lately been painfully excited by the accident which has befallen Mr. Cocking—the following drawing and description of the Parachute of the unfortunate experimenter, and narrative of his descent, cannot fail to be acceptable to our readers.

Mr. C. is the first since the day of Garnerin who has had the temerity to venture a descent in a Parachute. On the occasion of the descent of the last named æronaut, about thirty years ago, it was remarked by many scientific men present, that the great oscillation which took place from the moment the machine expanded till it reached the earth, must have been attributable to some defect in the principle of its construction.

Mr. Cocking, a gentleman who has made the science of aërostation his study for many years, and who has also delivered public lectures on the subject, was present at Garnerin's descent, and has since devoted much time and labour to discover the errors in the parachute then employed. Like all inventors, he met with numerous failures and disappointments, but at length his efforts were crowned with success, at least as far as experiments could prove; and we believe the circumstance which first led him to adopt the form now about to be used, was the accidental dropping an umbrella from a balcony. The umbrella fell the first few feet with the handle downwards, but after several oscillations its position was reversed, and having become inverted, it fell steadily to the ground.

Mr. Cocking from this occurrence felt convinced that the proper shape of such an instrument was exactly opposite to that used by Garnerin, and that it should be a convex, instead of a concave surface, or a cone with its apex downwards.

Garnerin's parachute was made of merely a circular piece of canvass, with cords attached at short intervals around the outer edge, all coming to one point below, to which the car was suspended. Now this altogether weighed so little, that any common balloon was capable of ascending with it; but to keep the new instrument in its proper shape, numerous large hoops, wooden braces, and a variety of other apparatus, were necessary, which rendered

the whole of so great a weight, that the persevering inventor, after all his labours, saw his model lie useless for years.

On learning the intention of the proprietors of Vauxhall to build a balloon of extraordinary dimensions, Mr. Cocking perceived an opportunity for introducing his parachute, and lost no time in communicating to them his plan. All difficulty was, however, not removed, for the natural question arose as to what was to become of the balloon after the parachute should be detached from it, the custom having previously been to construct one of cheap materials merely to serve for the one ascent, and which was generally either lost or destroyed, having frequently been blown out to sea, for no aëronaut could be found who would undertake to ascend and suffer so great a weight to be instantaneously separated from the balloon. Mr. Green, however, stated his opinion that the object might be accomplished without accident to the aëronaut, and removed all difficulty by offering to make the ascent himself. All was soon arranged, a new parachute of extraordinary dimensions constructed, and Monday, July 24th, fixed for the experiment. Three hoops, the largest of which is 107 feet in circumference, connected by ten light spars of wood, form the frame-work of the machine. These are strengthened by a series of small lines stretching from the upper hoop to the lower, and the whole is covered by a fine cloth; the latter consists of twenty-two gores, 59 inches at their greatest diameters, and gradually diminishing to 11 inches, which, when sewed together, form a cone at an angle of 30°.

The car is of wicker, and its attachment to the lower hoop resembles that of the car balloon, which always retains its perpendicular whatever movement there may take place in the machine above. A strong rope will descend from the hoop underneath the car of the balloon, and passing through an iron ring on the top of the main centre cord of the parachute, will ascend on the other side, and be made fast to the instrument commonly used

by Mr. Green for liberating the balloon. From this a thin cord will hang down to the car of the parachute, and thus give Mr. Cocking the opportunity of making the separation at any moment he may deem favourable. The surface exposed to the action of the air is 124 square yards, and the weight of the apparatus 223 lbs. This added to Mr. Cocking's weight, viz., 170 lbs., give the total of 393 lbs.; but it is calculated that the parachute is capable of descending with safety with a weight considerably greater, which is of course all in favour of the success of the experiment. The rate of the descent, as nearly as can be calculated, will be about 10 feet in a second, or six miles and a half an hour; but this will of course, in some measure depend on the state of the atmosphere.

The ascent took place from Vauxhall Gardens, July 24, towards six o'clock, the Messrs. Green entered their balloon, which was allowed to ascend to an altitude of about forty feet, that the parachute might be brought directly under it, and securely fixed. It was seven o'clock before all the preparations were completed, and in about half an hour afterwards, a tube or pipe of linen was lowered by the Messrs. Green from the car of their balloon through the orifice in the parachute, and past the basket in which Mr. Cocking was to sit. This was for the conveyance of the ballast it is found necessary to discharge on the ascent of the balloon, and which, if it had been thrown out in the usual manner would have lodged in the parachute. All the preparations having been completed, Mr. Cocking (having previously stripped off his coat as too cumbersome, and put on a light jacket) stepped into the car amid the acclamations of the company; the cords were loosened, and the balloon and its attendant parachute mounted into the heavens amid the renewed cheering of the crowd. The early part of the afternoon had been remarkably fine and clear, but about this time (half past seven) the sky had become somewhat overcast, and a breeze had sprung up. No apprehensions however, were entertained, and the scene at that moment was as gay and cheerful as it is possible to imagine. The balloon had hardly attained an altitude of two hundred feet when the tube destined for the escape of the ballast from the car above, detached itself by some means or other from the basket of Mr. Cocking, and floated like a riband in the air.

Mr. R. Underwood, of Regent street, followed on horseback in the direction taken by the balloon, to witness, if possible, the descent of the parachute, and from that gentleman we have learned the melancholy details which follow. Mr. Underwood was in the neighbourhood of Blackheath when he saw the cord which attached the parachute to the car severed. The parachute, thus left to itself, descended with the utmost rapidity, and swayed from side to side in the most fearful manner. Mr. Underwood anticipated the worst. In a few seconds, the dreadful oscillations still continuing, the basket which contained the unfortunate aëronaut broke away from the parachute, and Mr. Cocking was precipitated to the earth from a height of several hundred feet. Mr. Underwood immediately spurred his horse, and arrived in a field near Lee, where several labourers had picked up the parachute. They would not believe that a man had fallen with it, but on Mr. Underwood's explanations, and an offer of five guineas to whoever should find the body of Mr. Cocking, they commenced a diligent search. After traversing four fields they heard groans proceeding from a field called Burnt Ash, near Lee, and on going in that direction, they found the unfortunate Mr. Cocking literally dashed to pieces! and just as they were loosening his cravat, he breathed his last in their arms.

When the parachute reached the earth it was a confused heap covering the basket and body of Mr. Cocking, and all the ribs and tubes were broken into fragments.

The accompanying sketch exhibits the parachute at three stages of its descent. First, immediately after the experimenter separated from the car; next, at the time when the collapse took place from the weight and pressure of the atmosphere; and last, when it approached near the ground.

The following is Messrs. Green and Spencer's narrative.

In consequence of the sad and fatal catastrophe which has befallen the late Mr. Cocking, I feel myself called upon to communicate to the public the whole of the particulars of my ascent with the Vauxhall Balloon, taking up with me Mr. Cocking in his parachute. The inflation commenced about twelve under the able direction of Mr. Hutchinson, the engineer of the London Gas Company, and was completed by five o'clock. Prior to the parachute being attached to the balloon I caused a trial to be made with the view of ascertaining whether the buoyancy of the latter was sufficient to carry up the former with safety. The result of this trial was, after some arrangements with respect to the ballast, of which I was compelled to give out about 650 lb. in weight, had been effected satisfactorily. The abandonment of this large quantity of ballast I found to be absolutely requisite in order with safety to commence the ascent. The balloon was then allowed gently to rise a sufficient height to be conveyed over the parachute; but in consequence of the great and unavoidable delay which was necessarily caused in affixing the two machines, the gas in the former became very considerably condensed, from a reduction of temperature. It, thereupon, became a matter of compulsion that I should get rid of 100 lb. more of ballast, which I emptied out of the bags through a tube, constructed of canvass, and about fifty feet in length. The object in having this tube was, that any ballast I might deem it advisable to throw out during our voyage should take such a course as would clear the broadest expanse of the parachute. The connexion between the balloon and the parachute was at length completed by the rope of the latter being made fast to the liberating iron by which Mr. Cocking was to free himself from the balloon.

It is but justice to myself I should here state, that I had on several occasions expressed my determination not to liberate the parachute from the

balloon, upon the ground, setting aside any other consideration, that I might select a moment for severance when Mr. Cocking was not altogether prepared or ready for his descent, and therefore if any accident were to accrue to him, that I of course should be regarded as the responsible party, to whom blame would naturally attach.

Every thing being in readiness, about 25 minutes to eight o'clock, Mr. F. Gye, gave the signal for the whole of the apparatus to be released from its trammels, and we instantly rose very steadily, taking an easterly course.

Mr. Cocking always desired that we should ascend to an elevation of 8,000 feet, about one mile and a quarter, at which height he proposed to detach himself from the balloon, and commence his descent. Finding, therefore, that our upward progress was very slow, I requested Mr. Spencer to discharge some more ballast, and he accordingly threw the contents of a bag weighing 20 lb. through the tube already named. This proving of little avail, I directed a second and then a third bagful to be got rid of by the same means.

At this period we were floating over the Surrey Zoological-gardens, at an elevation of about 2,000 feet. It was at this moment that a portion of the lower end of the ballast-tube became detached, a circumstance which was caused by the occasional swinging to and fro of the parachute. This accident led to the inconvenience which I had forseen some days before the ascent, and which led to the adoption of the tube, and of that of rendering it extremely difficult for us to discharge the ballast without its falling into the parachute.

Our inability to do this as we were then situated I communicated to Mr. Cocking, adding that under the circumstances it was impossible for us to rise any higher unless we were to attempt to throw the ballest in bags beyond the outer spread of his machine, a course of procedure which we considered to be attended with much danger to any person who might chance to be beneath, but that we would, if he wished it, make the experiment as soon as he had cleared the houses. Mr. Cocking replied "Very well, it is of no consequence; if you think I have time to rise as high as I want, and to descend before dark." I remarked, "I think you have, and you will then also have a more open country for the descent." We now continued to glide along guided by the pleasure of the wind at nearly the same elevation until we had cleared all buildings. During this time Mr. Spencer and myself were busily engaged in dividing our ballast into small parcels, so that we might be able to throw them over without injury to the parachute.

As soon as we found that we had arrived over the fields, and presuming that no danger could arise from the falling of the ballast, we quickly began to relieve ourselves of that essential commodity. In doing this our anxiety respecting any of it lodging in the parachute was much relieved by finding that that machine continually swung backwards and forwards, evidently occasioned by the operation of the currents through which we passed, so then we were enabled without any difficulty to cast away the bags without damage to the vehicle immediately below us. We continued to discharge ballast until we had lessoned our quantity by 50lb., in addition to that already sent over. The balloon now began to rise, and soon entered a tier of clouds, when we lost sight of the earth. So great, however, was the resistance offered by the parachute to this denser atmosphere that we were again obliged, in order to attain the elevation Mr. Cocking pressed for (that gentleman considering that the greater the distance he had to fall, the greater would be the atmospheric pressure under the parachute, and therefore the easier descent) to rid ourselves of 400lb. more ballast, and even

then, we only arrived at the height of 5,000 feet, which is a trifle less than a mile. We were still 3,000 feet lower than Mr. Cocking's desired elevation.

"Whilst these operations were going on, Mr. Spencer and myself held a conversation with our appended neighbour and friend, which was entirely confined to the progress we were making upwards, Mr. Cocking manifesting much anxiety, and wishing to be informed how we were rising, requesting to know when every additional elevation of 500 feet was accomplished.

As soon as we had attained the height of 5,000 feet I told him that it would be impossible for us to get up as high as he desired in sufficient time for him to descend by the light of day. Upon this Mr. Cocking said, "Then I shall very soon leave you; but tell me whereabouts I am!" Mr. Spencer, who had a few minutes before caught a glimpe of the earth, answered, "We appear to be on a level with Greenwich." I then asked him if he felt himself quite comfortable, and whether he found that the practical trial bore out the calculation he had made? Mr. Cocking replied, "Yes; I never felt more comfortable or more delighted in my life." Shortly afterwards Mr. Cocking said, "Well, now I think I shall leave you." I answered, "I wish you a very good night and a safe descent, if you are determined to make it, and not to use the tackle."

I should here observe, that with an anxiety to prevent any accident arising in the event of the violence of the wind rendering it impossible for a descent to be attempted, an apparatus had been constructed under the direction of Mr. F. Gye, to afford us the facility of assisting Mr. Cocking to haul himself up into the car of the balloon, and that this is the tackle to which I thus alluded.

Mr. Cocking to this question made no other reply than "Good night, Spencer; good night, Green."

At this instant I desired Mr. Spencer to take fast hold of the ropes and like myself to crouch down in the car. In consequence of being compelled to keep hold of the valve line, of course I had but one hand which was available for the purpose of safety. With that hand, fortunately, in the perilous situation into which we were speedily thrown, I was able to maintain my position.

Scarcely were these words uttered before we felt a slight jerk upon the liberating iron, but quickly discovered, from not having changed our elevation, that Mr. Cocking had failed in his attempt to free himself. Another but more powerful jerk ensued, and in an instant the balloon shot upwards with the velocity of a skyrocket.

The effect upon us at this moment is almost beyond description. The immense machine which suspended us between "heaven and earth," whilst it appeared to be forced upwards with terrific violence and rapidity through unknown and untravelled regions, amidst the howlings of a fearful hurricane, rolled about as though revelling in a freedom for which it had long struggled, but of which until that moment it had been kept in absolute ignorance. It at length, as if somewhat fatigued by its exertions, gradually assumed the motions of a snake working its way with astonishing speed towards a given object. During this frightful operation, the gas was rushing in torrents from the upper and lower valves, but more particularly from the latter, as the density of the atmosphere through which we were forcing our progress pressed so hea-

vily on the valve at the top of the baloon as to admit of comparatively but a small escape by that aperture.

At this juncture, had it not been for the application to our mouths of two pipes leading into an air-bag with which we had furnished ourselves previous to starting, we must within a minute have been suffocated, and so, but by different means, have shared the melancholy fate of our friend.

This bag was formed of silk, sufficiently capacious to contain 100 gallons of atmospheric air. Prior to our assent the bag was inflated, with the assistance of a pair of bellows, with 50 gallons of air, so allowing for any expansion which might be produced in the upper regions. Into one end of this bag were introduced two flexible tubes, and the moment we felt ourselves to be going up, in the manner just described, Mr. Spencer, as well as myself, placed either of them in our mouths. By this simple contrivance we preserved ourselves from instantaneous suffocation, a result which must have ensued from the apparently endless volume of gas with which the car was enveloped. The gas, notwithstanding all our precautions, from the violence of its operation on the human frame, almost immediately deprived us of sight, and we were both, as far as our visionary powers were concerned, in a state of total darkness for between four and five minutes.

As soon as we had partially regained the use of our eyes, and had somewhat recovered from the effects of the awful scene into which, from the circumstances, we had been plunged, our first attention was directed to the barometer. I soon discovered that my powers had not sufficiently returned to enable me to see the mercury, but Mr. Spencer found that it stood at 13-20, giving an elevation of 23,384 feet, or about four miles and a quarter.

I do not conceive, from the length of time I had been liberating the gas that this was any thing like our greatest altitude for we were evidently effecting a rapid descent. This impression is corroborated by a rough calculation, which leads me to believe, knowing the customary rate at which the gas makes its escape, taken in consideration in conjunction with the length of time I had been pulling the valve-line, that we had lost at least 30,000 feet of gas or 180,000 gallons, a total of 5,000 feet more than my own balloon will contain.

It may be regarded as somewhat surprising that not a larger quantity had evaporated, especially when the size of the valves are considered, that at the top being nearly three feet in diameter, whilst the one at the neck of the balloon is upwards of two feet. The extreme rapidity with which we ascended, coupled with consequent pressure of the atmosphere on the upper part of the machine, necessarily prevented much escape from the top valve. The same cause also forced an extraordinary emission from the opening at the neck, and I am decidedly of opinion, had it not positively happened that the proprietors permitted this latter valve to be increased from 18 to 25 inches in diameter, that the baloon must have burst, and my companion as well as myself, been hurled headlong into eternity.

As I have stated, we were now rapidly on the descent, having got rid of all the unusual annoyances to which I have referred ; and finding that we were proceeding downwards with the ordinary calmness and steadiness, although with much speed, we hastened to empty two tin vessels of water which we had taken up for the purpose, and to charge them

with the atmospheric air through which we were then descending. Our desire was to effect this object at our greatest altitude, but from the circumstances which I have detailed we were unable to accomplish that end, and when the vessels were filled, the mercury in the thermometer had ascended to 17-50, or an elevation of 16,632 feet, about three miles.

When we had accomplised this matter, finding ourselves suffering severely from cold, we referred to the thermometer, which stood at 28, four degrees below the freezing point.

We were at this period apparently about two miles and a half above a dense mountain of clouds, which presented the appearance of impenetrable masses of dark marble, whilst all around us were shed the brilliant rays of the setting sun. We continued to descend with great rapidity, and as we approached the clouds, that velocity considerably increased. At this time so large had been our loss of gas, that the balloon, instead of presenting to our sight its customary round and widely expanded form, now merely looked like a comparatively small parachute, or half dome, without any aperture in its centre. We had parted with at least one-third of our gas, and were as far beneath the balloon itself as 50 or 60 feet.

Recollecting the late hour at which we quitted Vauxhall, I now began to be anxious about the time, and on applying to Mr. Spencer, ascertained that it wanted not more than a quarter of 9 o'clock. From this I was aware, notwithstanding in our then position we were blessed with a magnificent light, that on emerging below from the clouds darkness would have assumed her sable hue over the earth, and that we should have much difficulty, therefore, in ascertaining the nature and character of the country, supposing us to be over the land, on which we must effect our final descent. I, consequently, became extremely anxious to make our way through the clouds as quickly as possible, which having done we proceeded, until we had reached within some 300 feet of the ground, when we found it requisite, from our inability to ascertain the nature of the ground, the whole country beneath us offering the appearance of thick woods, to cast out every article of ballast and movable matters, even to ropes and empty ballast-bags, in order to prevent us from coming in contact with what was supposed to be trees. After calling out for some time, and hanging out the grapnel, we heard voices in reply, and the parties speedily drew us to a safe place of landing, which proved to be close to the village of Offham, near Town Malling, seven miles west of Maidstone, and 28 from London.

The balloon was packed, and conveyed in a cart to Town Malling, where we were most hospitably treated and provided with beds by the Rev. Mr. Money, who, singular to relate, informed me that he is the son of Major Money, the aeronaut, who, on the 23d July, 1785, ascended from Norwich, and fell into the sea 20 miles off Lowestoff.

At half-past ten o'clock this morning we quitted Town Malling, and it was not until our arrival at Wrotham, at which place I inquired whether they had heard where Mr. Cocking had descended, that I became acquainted with the unexpected and melancholy result of his experiment.

I trust it is needless for me to say, how deeply the feelings of Mr. Spencer and myself were harrowed up by the sad intelligence thus conveyed to us.

It is only due to the late Mr. Cocking I should add, that throughout the whole of our voyage, up to the moment when he released himself from the balloon, he displayed the greatest courage and fortitude, and the expression of his features, and the light and joyous, although earnest way in which he made his inquiries and conversed with us, manifested his great satisfaction that at length a theory to which he had devoted the last 25 years of his life was about to be triumphantly put to the test.

We were up about one hour and twenty minutes.

Individually my opinion was, that having withstood the difficulties and severe pressure of the atmosphere in its ascent, Mr. Cocking's parachute would accomplish its descent with perfect safety."

The following evidence of Prof. Airy, before the coroner's inquest, will furnish all the additional information of interest in a scientific point of view.

Professor G. B. Airy, the Astronomer Royal of Greenwich, stated that he saw the balloon shortly after its starting, it was then travelling with the wind. It shortly afterwards altered its course. The parachute he saw, and was struck with its appearance. He saw it through a common telescope of a 12-times magnifying power. He was sitting with some ladies of his family at the drawing-room window, looking at the balloon; suddenly he heard a loud shout of persons in the park, and he knew that the parachute had got away. It had been stated that the parachute kept its shape for 40 or 50 seconds, but he was quite sure that it did not retain its shape for more than four seconds, for he put his eye instantly to the glass and found it in a collapsed state. He was convinced there had been no turning over. Had it been turned over the basket would have been displaced. He observed the sides of the parachute flickering backwards and forwards. His opinion as to the efficacy of the construction was, there was not sufficient account taken in such construction as to unavoidable disturbances, and the tendency of the air was to force it in at the side, and the pressure of the air would in case of its getting out of shape only aggravate the evil, and the experiment must fail. This must therefore be considered as a construction quite wrong, and he should have thought that a person with common sagacity might have been aware of this. With regard also to the tin tube of which the circular ring was formed, it was hollow throughout, it was without stops, which would have strengthened it, and consequently as bad a thing as could have been used. Had stops been introduced it would have saved it from a great deal of the tremor to which the pressure of the atmosphere exposed it. Had the weight been a little greater in the top it would probably have come down sideways, and turned upside down. In this respect it was very badly constructed, and very inferior in many respects to parachutes of the old construction.

A jurer said it had been considered that if it resisted the force of the atmosphere in ascending it would be perfectly safe to come down with it. He wished to know what was the professor's opinion on this point?

Professor Airy said his opinion was quite the reverse. The air, by pressing upon the canvass, would keep the ring of tin to which he had alluded expanded, but the force of the air under would have the effect of bending it, and thus allowing the parachute to collapse.

LUNAR OCCULTATIONS FOR PHILADELPHIA, OCTOBER 1837.						Angles reckoned to the right or westward round the circle, as seen in an inverting telescope. ☞ For direct vision add 180° ☜	
Day.	H'r.	Min.	Phase	Star's name.	Mag.	from Moon's North point.	from Moon's Vertex.
7	6	59	Im.	b, Saggittarii,	5,	70	74
7	8	0	Em.			328	344
15	10	52	Im.	65 Arietis,	6,	112	62
15	12	2	Em.		"	311	270
31	4	11	Im.	d Scorpii	1,	94	123
31	5	25	Em.			250	290
LUNAR OCCULTATIONS IN NOVEMBER, 1837.							
11	4	52	Im.	π Arietis	,5,	146	96
11	5	37	Em.			267	214
11	8	6	Im.	ξ^3 Arietis	,6,	146	94
11	9	7	Em.			280	235
14	11	13	Im.	c Tauri	,4.5,	43	344
14	11	51	Em.			343	286

Meteorological Observations for June, 1837.

Moon.	Days.	Therm. Sun rise.	Therm. 2 P.M.	Barometer. Sun rise.	Barometer. 2 P.M.	Wind. Direction.	Wind. Force.	Water fallen in rain.	State of the weather, and Remarks.
				Inches	Inches			Inches.	
	1	64°	82°	29.85	29.96	SW.	Moderate.		Partially cloudy—do.—auro. bor.
	2	62	85	96	96	SW.	do.		Lightly cloudy—clear, aur. bor.
⊗	3	66	86	75	60	S.	Brisk.	.3	Clear—shower.
	4	67	76	55	56	W.	do.	.28	Clear—cloudy—thunder shower.
	5	62	74	60	66	W.	do.		Partially cloudy—do. do.
	6	59	82	65	65	W.	Moderate.		Clear—do.
	7	61	84	50	45	W.	do.		Clear—do.
	8	60	62	45	55	NE.	do.		Cloudy—do.
	9	61	72	70	80	E.	do.		Cloudy—do.
	10	54	74	86	90	E.	do.		Cloudy—do.
☾	11	55	68	95	90	E.S.	do.		Cloudy—do.
	12	64	72	90	84	S.	do.		Cloudy—do.
	13	60	72	75	75	S.NW.	do.	.14	Rain—partially cloudy.
	14	58	70	75	80	S.	do.		Partially cloudy—clear.
	15	58	63	70	70	N.	do.	.25	Cloudy—rain.
	16	54	74	70	72	W.	Brisk.	.17	Clear—do.—shower.
	17	52	70	72	72	NW.	do.	.5	Partially cloudy—shower.
⊛	18	54	67	70	80	NW.	do.		Cloudy—flying clouds.
	19	51	69	80	80	SW.	do.	.80	Clear—cloudy—rain in night.
	20	60	71	00	00	W.	Moderate.		Cloudy—flying clouds.
	21	60	72	00	5	W.	do.	.10	Rain—flying clouds.
	22	58	77	10	75	W.	do.		Flying clouds—clear.
	23	58	77	30.00	30.00	S.	do.		Cloudy—lightly cloudy.
	24	66	70	29.90	29.85	S.	do.		Cloudy—rain.
☽	25	69	77	77	77	W.	do.	.30	Cloudy—shower.
	26	63	81	90	95	W.	do.		Fog—lightly cloudy.
	27	66	82	95	80	W.	do.	.7	Cloudy—shower.
	28	68	80	90	30.00	W.	do.		Cloudy—do.
	29	66	68	30.00	00	S.	do.	.45	Cloudy—rain.
	30	64	86	29.80	29.80	W.	do.	.2	Cloudy—clear—rain.
	Mean	61.06	75.06	29.68	29.71			2.66	

Thermometer.			Barometer.		
Maximum height during the month	86. on 3d & 30th.	.	30. on	23d 28th 29th.	
Minimum do. .	51. on 19th . .	.	29. on	5th 20th 21st.	
Mean do. .	68.06 . . .	.	29.69.		